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VOLUME XV No. 57 MAY 1964

CONTENTS

ARTICLES

ON THE FOUNDATIONS OF STATISTICS 1
by Ian Hacking

THE INFERENCE OF FUNCTION FROM STRUCTURE IN FOSSILS 27
by M. J. S. Rudwick

REASON AND EXPERIMENT IN RELATION TO THE SPECIAL RELATIVITY THEORY 41
by Herbert Dingle

REVIEWS

Explanation in Social Science 62
article-review by I. C. Jarvie

Collingwood Reconsidered 72
article-review by B. T. Wilkins

The Myth of Metaphor. By Colin Murray Turbayne 78
reviewed by W. Mays

ANNUAL CONFERENCE OF THE BRITISH SOCIETY FOR THE PHILOSOPHY OF SCIENCE 83

ANNOUNCEMENTS 85

ERRATUM 88

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The British Journal for the Philosophy of Science

VOLUME XV May, 1964 No. 57

ON THE FOUNDATIONS OF STATISTICS *

IAN HACKING

THE problem of the foundations of statistics is to state a set of principles which entail the validity of all correct statistical inference, and which do not imply that any fallacious inference is valid. But most statistical inference is concerned with a special kind of physical property. Most of the foundations depend on the definition of this property, and this paper is devoted to that problem. It will,

(i) Indicate the property I have in mind;
(ii) Say what it is a property of;
(iii) State, as postulates, three well known characteristics of this property;
(iv) Urge a fourth postulate and urge it as a cornerstone in the foundations of statistics;
(v) Explain the theory of statistical tests in terms of these four postulates, and establish the respective domains of two hitherto controversial theories of testing.

The theory here developed can be applied to all kinds of statistical estimation, but this extension of it will not be discussed.

Jeffreys's *Theory of Probability* was for long the only major work using the axiomatic techniques familiar to logicians in order to derive, criticise, and contribute to the procedures of statisticians. Underlying his theory is a logic of what he calls probability; in his usage the probability of a proposition, given some information, is the degree of confidence you should place in that proposition if you are given only that information. To apply his theory in statistics he must postulate the degree of confidence you ought to have when you are almost completely ignorant, and his assumptions in this regard have always seemed very dubious. The present paper will use a much weaker underlying logic than that constructed by Jeffreys, and will not need any assignment of prior probabilities. Its novel feature is its fourth postulate, which will state a fundamental connection between long run frequency and what Jeffreys would call probability. The result is very much in the spirit of Fisher's views on statistical inference; certainly the key concepts of this paper stem from his work.

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* Given to the British Society for the Philosophy of Science, 9 December, 1963.

1 *The Property*

A coin, when tossed in a device, more often falls heads than tails. Overall, in the long run, the frequency of traffic accidents in a foggy city is pretty constant. Plants of one particular variety of maize more frequently mature before plants of another variety, rather than the other way about. Such examples are familiar. All indicate properties of some parts of the world. The properties can be experimented on, and altered, as by traffic safety programmes. There are indeed sceptics who say the long run frequency of heads is no property of anything. They have this much right on their side: the property has never been defined. But the property can be indicated and it is a serious conceptual problem to define it.

First, what to call the property? 'Frequency in the long run' seems appropriate. At least this name makes plain a problem: 'the long run'. As is well known, many statisticians use the word 'probability' as the name of this physical property. No matter what you think the meaning of this word, and no matter how you think it ought to be used, it is an undeniable fact that the word is used by some statisticians as a name for a physical property, and nothing but a physical property. However I shall not so use the word: in fact, I shall not use the word 'probability' at all. There is nothing wrong with using it, but I do not wish to invite mere verbal controversy.

'Frequency in the long run' is all very well, but it is a property of the coin and tossing device, not only that, in the long run, heads fall more often than tails, but also that this would happen even if in fact the device were dismantled and the coin melted. This is a dispositional property of the coin: what the long run frequency is or would be or would have been. Popper calls it a 'propensity' of the coin, device, and situation. Now if a wine glass would break, or would have broken, or will break, when dropped, we say the glass is fragile. There is a word for the active event, and another for the passive dispositional property. It will be convenient to have a word for our dispositional property—a brief way of saying what the long run frequency is or was or would have been. ‘Probability’ is often so used, but I eschew it here. So I shall resurrect a good seventeenth-century word which seems sometimes to have been used in just this sense—*chance*. This follows several recent recommendations in the statistical literature. So when I say the chance of getting heads on a toss of this coin from a certain device is ·7, I mean at present no more than that the frequency in the long run is or would have been (etc.) ·7. No more. Our task is to define this property. So far it has only been indicated.

There are, incidentally, a lot of deep reasons for wanting a dispositional term like ‘chance’. For instance, although static frequencies can be found in the world, in more interesting cases, ‘frequencies’—propensities—are continually changing, and we need the notion of the chance at any instant, when it is nonsense to speak of a long run. We must frame a definition of chance applicable to continuously changing chances as well as to static ones.

2 *What is ‘Chance’ a Property of?*

Von Mises said chances are properties of infinite sequences of a special kind; Fisher speaks of hypothetical populations. A much more naïve answer stands out. The chance of heads on a toss of this coin is a property of the coin, tossing device, and surroundings: what Popper calls the ‘arrangement’ and von Mises the ‘experimental set-up’. Likewise, the chance of 20 car accidents in a foggy night in Birmingham is a property of some part of the world, hard though it is to specify. We have no general term in English for this sort of thing. Following von Mises's translators I shall call it a *chance set-up*. We also need a term corresponding to the toss of a coin, the arrival of a foggy night. *Trial* has been used, and I adopt it.

A *chance set-up* is a device or part of the world on which might be conducted 1 or more trials, experiments, or observations; each particular trial must have a unique *result* which is a member of a definite *class of possible results*.

This is mere introductory terminology. But note that one set-up may provide many kinds of trial. If a particular coin and tossing device were so made that on any toss, the coin must fall heads or tails, one might

(i) toss up and note result: there are 2 possible results, H, T;
(ii) toss up thrice and note number of heads: 4 possible results;
(iii) toss up thrice and note arrangement of H and T in the outcome: 8 possible results;

There can also be conditional trials:

(iv) toss up and abandon trial if coin falls T, otherwise toss up again and note H or T on 2nd toss. 2 possible results, conditional on getting T first.

It is formally much easier to discuss sets of results rather than individual ones. Given a kind of trial and a set of mutually exclusive possible results, an *outcome* shall be any set of possible results. For convenience I shall not here distinguish between a result and its unit set, (the outcome whose sole member is this result). For any particular kind of trial, the set of all possible results shall be called the sure outcome, Ω.

The chance of getting an outcome E on a trial of kind K from chance set-up X is just the chance of getting one or the other of the members of E as a result of a trial of kind K. This chance of an outcome E on trials of kind K is the very property of the set-up which we want to consider.

3 *The kind of definition*

The task of defining the physical property of 'chance' or frequency in the long run is not to find an equivalent form of words, as when a chiliagon is defined as a 1000-angled plane figure. The problem is to devise a postulational definition. Adapting one of Quine's phrases, let us say that a term occurs essentially in a valid inference if replacing that term by another of the same grammatical type can make the inference invalid. Then the aim of a postulational definition is to state a set of postulates which entail the validity of all those correct inferences in which only logical terms, and 'chance', 'outcome' and related terms, do occur essentially; it must also avoid validating any incorrect inference.

Cramer's account of statistical inference is essentially a postulational definition. In addition to some standard axioms, he suggests we postulate that if, under certain conditions, the chance of a given event is small enough, then it is 'practically certain' and in practice 'certain' that the event does not occur. This postulate is not only false: it fails to validate much statistical inference.

Braithwaite's theory is also postulational: in fact, he offers an infinity of possible definitions. The key postulate of each is a k-rule of rejection, where k is a parameter characterising the postulate.[1] A k-rule postulates the conditions under which a statistical hypothesis must be provisionally rejected in accord with current data. 'k' indicates the stringency of the rule. This theory, too, is inadequate for a good deal of statistical inference. It is also weird. Let $k_1 \neq k_2$. Then a k_1-rule defines one physical property, which we may call k_1-chance, and a k_2-rule defines k_2-chance. There are two possibilities. (i) k_1 and k_2 chances of the same thing may differ, or (ii) they cannot. If (ii), then if we provisionally reject (by the k_1-rule) the hypothesis that the k_1-chance is p, it seems correct to reject, provisionally, the hypothesis that the k_2-chance is p. Braithwaite's rules are so devised that, if this is correct, we may on any data provisionally reject, for any k whatsoever, all but one of the k-hypotheses: we need only hunt around for a sufficiently stringent value of k_1. This conclusion completely vitiates Braithwaite's theory. But if we take alternative (i), the theory is even more mysterious. What must be the case in the world for the k_1-chance of something to be really (not just in our provisional opinion) p, while the k_2-chance is not?

The forthcoming theory uses an explicit postulational definition. One point is especially worthy of notice. We aim to circumscribe the dispositional property corresponding to 'frequency in the long run'. 'Long run' is notorious. There are two alternatives. First, we might define 'long run': that was von Mises's method. Second, we may strive for a set of postulates which hold true of long run frequencies under any construction people have cared to put on 'long run'. We shall take the second course. It seems to be the right one: a lot of inferences about long run frequencies are agreed to by persons who quarrel violently over 'long run'. Hence the validity of the inferences may be pretty well independent of what has been intended by 'long run'.

The forthcoming theory is rather outside the mainstream of current research in statistics: it ignores decision theory. I shall mention two of several reasons for this. First, we are concerned with defining a physical property. All the considerations currently used in decision theory take this property for granted, so it is hardly the tool for analysing the property. Second, decision theory must employ several other concepts, such as utility, whose definition is dubious. It is to be hoped that the definition of chance need not import such further difficulties. However, I have promised to apply the present theory to the theory of testing hypotheses: this will only be a measure of the success of our definition, and it is not intended as a complete theory of testing, which probably must be constructed in decision theory.

---

[1] R. B. Braithwaite, *Scientific Explanation*, Cambridge, 1953, Chapter vi

4 *Kolmogoroff's axioms*

Some characteristics of chances are well known. We assume that for any kind of trial, the class of outcomes forms a field: unions, intersections, and complements of outcomes are also outcomes. The following three characters are usually admitted to pertain to frequencies —be they frequencies in a finite or infinite run. For brevity, we represent the chance of outcome E on trials of kind K on set-up X, by P(E), where K and X are made plain by context. Then for any K and X:

(1) $0<P(E)<1$
(2) $P(\Omega) = 1$
(3) if $E \cap F = 0$, $P(E)+P(F) = P(E \cup F)$

Those are the generally accepted Kolmogoroff axioms. I take them for granted as our first three postulates about chance. If there are infinitely many possible results, the third postulate is extended to all countable sums of disjoint outcomes (and we assume a countable union of outcomes is also an outcome).

It is easy to define 'conditional chance' (the chance of event E, given that F does occur),

this is $P(E/F) = \dfrac{P(E \cap F)}{P(F)}$.

Likewise, it is easy to lay down a definition of independence. The whole 'calculus of probabilities' follows. Note we have given three axioms for 'frequency in the long run' without mentioning a 'run' of any specific length. Moreover we can establish well known connections between different kinds of trial without mentioning length of run. Consider two kinds of trial on a coin tossing device:

(i) toss and note H or T;

(ii) toss n times, note number of heads.

Let P(H) be the chance of head on trial of kind (i)

B(k, n) be the chance of k heads on trial of kind (ii).

We may prove

$$B(k, n) = \frac{n!}{(n-k)!k!}[P(H)]^k[1-P(H)]^{n-k}$$

from our definition of independence without assuming that there could be any more than n tosses of the coin. This is very important: the Kolmogoroff axioms are simply independent of ' length of run '.

5 *Distributions*

The next important notion is that of a distribution. It will not be used over-much in this paper, but it is indispensable in statistics: there is no testing of a statistical hypothesis which does not essentially involve the notion of a set of possible distributions of chances. But here I shall introduce the matter under only the most elementary aspect.

In the coin tossing experiment we might state the chance of 2 heads in 4 tosses. But a more informative statement about this kind of trial would state the chance of 0 heads in 4 tosses, of 1 head, of 2, of 3, and of 4. This will be called the *distribution* of chances of outcomes for this kind of trial. More generally, for any kind of trial, a distribution of chances of outcomes will be a comprehensive assignment of chances to possible outcomes, which satisfies Kolmogoroff's axioms. The problem of practical statistics is to discover which distribution of chances provides the true description of a chance set up and kind of trial.

Often a distribution can be stated simply. If a trial consists of noting the number of heads in n independent tosses, the chance of any outcome can be computed from the value of P(H) for a single toss using the formula for B (k, n); hence one distribution can be briefly stated: ' independent trials, P(H) = 0·7 '.

If a trial has finitely many possible results it will always be possible to assign a chance to every possible outcome; if there are more than finitely many possible results, this may not be possible. The conditions under which it is possible, and in which it is not possible, have been closely investigated in measure theory. I shall consider only finite examples in what follows, but I would emphasise that the present theory must be capable of including the case of trials with infinitely many possible results. They are the most interesting in practice, but too many technicalities are required to treat them rigorously here.

It is, as has been said, an aim of practical statistics to discover which of several distributions in fact pertains to trials of a certain kind on a given chance set-up. By a *statistical hypothesis* I shall mean one of the form ' on trials of kind K from set-up X the distribution of chances of outcomes is D '. Nothing said so far gives any indication of when such an hypothesis might be acceptable. Our task is to discover relations between such hypotheses, and empirical evidence for them—such data as, say, that on trial T of kind K, the outcome was E. It has been rather generally supposed that the facts about frequency which validate inference from known frequency to hypotheses about the outcome of a particular trial, differ from the facts which validate inferences from known outcomes, to statistical hypotheses. I believe the same fact pertains to both.

6 *A Fact about Frequency*

In the coin tossing experiment, suppose it is known that in the long run H falls more often than T. Then, if this is all that is known, and you must guess H or T on the next toss, the best guess is H. In fact the hypothesis ' H will fall next toss ' is, it is universally agreed, better supported by this data than the hypothesis ' T '. It is agreed even by those who disagree on how to analyse ' more often in the long run '. Elsewhere I have recently argued that this is symptomatic of a general fact about frequency, and cannot be justified by more fundamental facts which differ from it in more than generality.[1] In particular, I argued that the good support for ' H ' cannot be justified by any claim that in the long run, a man who guesses ' H ' would guess rightly more often than if he had used another policy. Despite some defects in that paper, no one has yet even tried to fault the general thesis that ' H ' is best supported and that this is a fundamental fact about frequency and support. Now exactly what is this general fact?

My guess is as follows. Suppose we are considering two hypotheses, *h* and *h′*, and some body of data, *d*. I claim that so long as *h* is consistent with *d*,

---

[1] Ian Hacking, ' Guessing by Frequency ', *Proceedings of the Aristotelian Society*, 1963-4, 63, 55-70

if *h* & *d* implies that something, which is an instance of what happens rarely in the long run, happens on a particular occasion, while
h′ & *d* implies that something, which is an instance of what happens less rarely in the long run, happens on a particular occasion,

then, lacking other information, *d* supports *h′* better than *h*.

This rather cumbrous and obscure claim must be explicated. But I think it is true, and expresses a fundamental connection between frequency and support, which is not to be deduced from other propositions except those which differ from it only in generality. Note that it seems true whichever of the usual senses be attached to the ' long run '.

Like any other proposed postulate, the precise explication of this claim is to be tested by deriving many consequences which are probably true, and none which are probably false. There is, ultimately, no other way of supporting such a postulate except in terms of its consequences. Note one true consequence already: in the example above, ' H ' is better supported than ' T ', as desired. I shall submit that a precise statement of the cumbersome claim above *states the fact about long run frequency which is fundamental to all statistical inference, and that this fact is fundamental both to guessing what will happen, when frequencies are known, and to guessing frequencies, when what has happened on some individual trials is known.* The point of my previous paper was to prove that this fact does not derive from any considerations of long run success; it is a fact which provides the very definition of long run frequency, and is not something to be inferred from properties of long runs.

7 *Support*

A great deal can be said about the notion of support by data, but here I shall take it as undefined. In saying that *e* supports h_1 better than h_2, I mean that *e*, taken alone, does so, and not that when some other information is already known, discovery of *e* in some way corroborates or confirms h_1 better than h_2. The latter notion has been investigated, but we shall be concerned solely with the former, whose logical principles were first stated by Koopman.[1] Moreover, we shall rely on the logic of relative support, which is restricted to assertions of the form, 'e_1 supports h_1 at least as well as e_2 supports h_2'. The logic of absolute support, which covers assertions of the form, 'e supports h to degree p' is, to my mind, rather uncertain, but fortunately it will not be needed for the present enquiry. We shall write:

---

[1] B. O. Koopman, ' The Axioms and Algebra of Intuitive Probability,' *Annals of Mathematics*, 1940, 41, 269–292

$$h_1/e_1 < h_2/e_2$$

to mean that h_2 is at least as well supported by e_2 as is h_1 by e_1. Few explicit calculations of support will be performed in this paper, but it is useful to note propositions such as:

$$\text{if } h_1 \text{ implies } h_2,\ h_1/e < h_2/e$$
$$\text{if } e \text{ implies } h_2,\ h_1/e = h_1 \,\&\, h_2/e$$

and also the following thesis to be used in a couple of subsequent derivations:

thesis: if e implies h_3, and $h_1 \,\&\, h_3/e < h_2 \,\&\, h_3/e$, then $h_1/e < h_2/e$.
All these propositions are consequences of Koopman's logic.

There must be one caution about support. We shall be concerned with support for statistical hypotheses. Now suppose we have a class of possible hypothesis, all of the form '$P(H) = p$'. It might seem that the best estimate of $P(H)$ is some value p if and only if '$P(H) = p$' is the best supported hypothesis. But this is not true. For suppose, on given data, 6 hypotheses are possible: $P(H) = {\cdot}90$ or ·12 or ·11 or ·10 or ·09 or ·08. And suppose the last 5 hypotheses are equally probable according to this data, while the first is slightly more probable—in any sense at all which you care to give to 'probable'. Then there are strong reasons for saying that $P(H) = {\cdot}90$ is the best supported of the 6 hypotheses, but it is surely not the best estimate. For some purposes $P(H) = {\cdot}10$ might seem the best estimate, for it is more probable (in whatever sense was given to 'probable') that ·10 is close to the true value, than that ·90 is close to the true value. I cannot briefly explain exactly what a good estimate is, but this instance should show that what is best supported cannot be expected necessarily to coincide with the best estimate. Estimation cannot be studied without analysing 'closeness' to a true value. Support can. Support is the more primitive notion even if in practical affairs it is less useful.

8 *A law of likelihood*

Our first attempt to state a connection between frequency and support mentions hypotheses, data, and what is essentially a chance. In explicating it consider a special kind of proposition: call it a *joint proposition*. It is of the form

> 'Outcome E occurs on trial T of kind K on chance set-up X, and the distribution of chances of outcomes from trials of that kind is D.'

Hence a joint proposition is an ordered quintet $\langle X, K, T, E, D\rangle$—$\langle$set-up, kind of trial, particular trial, outcome, distribution$\rangle$. Now suppose it is possible to compute the chance of an outcome E, on the assumption that the true distribution is D. If this is possible, this will be called the *likelihood* of the joint proposition. The word and the idea are both Fisher's.[1]

Secondly, a kind of data must be specified. *Statistical data* will be data of the form, 'the distribution of chances of outcomes from trials of kind K, on set-up X, is a member of the class of possible distributions Δ; the outcome of trial T of kind K′ on X is contained in the possible outcome E′. Represent this by the sextet $\langle X; K, \Delta;\ T, K', E\rangle$. Two pieces of statistical data of the form $\langle X, K_1, \Delta_1;\ T_1, K_1', E_1\rangle$ and $\langle X, K_2, \Delta_2;\ T_2, K_2', E_2\rangle$ will be called *equivalent* if and only if (taking Kolmogoroff's axioms for granted) the two entail each other.

A joint proposition $\langle X, K, T, E, D\rangle$ is consistent with statistical data e if and only if there is some $\langle X, K, \Delta;\ T, K, E'\rangle$ equivalent to e and such that $D \epsilon \Delta$ and $E' \epsilon e$.

Finally we can explicate the connection between support and frequency, in the special case of discrete distributions, in which there are finitely many possible results. *The law of likelihood states:*

> If *h* and *h′* are joint propositions, and *e* statistical data, *e supports h better than h′ if h is consistent with e and has greater likelihood than h′*.

Aside from 'support', all the non-logical terms occurring in the statement of this law have been defined in terms of 'chance'. So if Koopman's logic is taken as the underlying logic, the law may serve as a fourth postulate about chance to be added to Kolmogoroff's three axioms. These four postulates provide for the foundation of much statistical inference and define, as much as we shall here define, the property of chance or frequency in the long run. The postulates are demonstrably consistent. Moreover, Kolmogoroff's axioms hold for any construction put upon 'long run', and the law of likelihood claims to explicate a connection between support and frequency in the long run which holds no matter what be intended by 'long run'. So chance has been defined without any special sense being attached to 'long run'.

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[1] Fisher applied the term 'likelihood' to statistical hypothesis in the light of experimental results. Here we apply it to propositions which assert, jointly, an hypothesis and a result. Barnard has a notion similar to, but different from, that of a joint proposition; he calls it the 'concrescence' of a statistical hypothesis with the statement about a result; he says this is not an ordinary conjunction, and distinguishes the likelihood of a 'concrescence' when the result is known, from its (numerically identical) likelihood when the hypothesis is known. See G. A. Barnard, 'Statistical Inference,' *Journal of the Royal Statistical Society*, B, 1949, 11, 115-39.

9 *Applications of the law*

The law applies, in a rather obvious way, to inferences from frequencies to guesses about a particular trial. Let there be two possible results, heads and tails, of tossing a coin; let this be a chance set-up in which tosses are independent, with P(heads) = ·7. Then considering any particular toss, T, say the next one, we frame 2 joint propositions, for short,

h_1: the outcome of toss T is heads; P(heads) = ·7
h_2: the outcome of toss T is tails; P (heads) = ·7.

The statistical data states independence, that P(H) = ·7, and that the outcome of trial T is heads-or-tails. Both h_1 and h_2 are consistent with the data; h_1 has likelihood·7; h_2 has ·3. So h_1 is better supported by the data than h_2. Since the data includes the fact that P(H) = ·7, it follows by the thesis on support, quoted earlier, that the hypothesis 'the outcome of toss T is heads' is better supported by the data than 'the outcome of toss T is tails'. Which is as desired.

Now take an inverse inference. Consider a coin tossing experiment. P(H) is unknown, but $0<P(H)<1$. The tosses are known or assumed to be independent, so the chance of k heads in n tosses is a function of P(H), viz. $\frac{n!}{(n-k)!\ k!} \cdot [P(H)]^k\, [1-P(H)]^{n-k}$. On a trial T of n tosses, exactly k tosses were observed. This completes the statistical data. There are infinitely many joint propositions essentially saying

h_p: 'the outcome of trial T of n tosses was k H; $P(H) = p$'

when p ranges from 0 to 1. For what value of p is the corresponding h_p best supported? It is easy to prove that the likelihood of h_p is maximum for $p = \frac{k}{n}$. So, using our thesis on support, the hypothesis best supported by the statistical data is $P(H) = k/n$. It is generally agreed that, on the stated data, *k*/*n* ought to be the best support hypothesis; the point of the above derivation is not to prove that this usual answer is incorrect, but to show that the law of likelihood provides the usual answer. This 'inductive' procedure provides the best defence of our proposed foundations.

Remark

The law of likelihood has been stated only for the case of finitely many results. It must be generalised to the infinite case. In that case likelihood may be 0, but the ratio of likelihood between two hypotheses can be defined under very general conditions which will not here be explained.[1] And there are strong reasons, aside from mathematical elegance, for extending the law to read: 'if *h* is consistent with *e*, and if the likelihood ratio of *h* to *h'* exists and is greater than 1, then *e* supports *h* better than *h'*.' However, accurate statement of these matters requires the apparatus of measure theory.

The law of likelihood has many forebears: perhaps its progenitor is D. Bernouilli.[2] But it must be distinguished from two current doctrines. First, it differs from Fisher's principle of maximum likelihood because that is a principle of estimation and ours is one of support.[3] I have remarked that 'best estimate' and 'best supported' need not coincide. One might accept our law, but still doubt Fisher's principle. Even if *k*/*n* is best supported, it might, for all I have argued, be a bad estimate of the chance of getting *k* heads in *n* tosses. There are reasons for denying Fisher's principle which do not impinge at all on the law of likelihood.

Second, our law differs from what Savage calls the likelihood principle, and which has been variously enunciated, from one point of view, by Fisher and Barnard, and from another point of view, by Birnbaum and Savage himself.[4] One statement of the principle says that likelihood must be the sole basis of statistical inference or decision. Our law is less dogmatic, since it considers only inference based on one kind of data, namely what I call statistical data; it is also more specific and open to refutation, for it states the precise relevance of likelihoods to that data. But of course the ideas are closely connected; perhaps accepting the law of likelihood could give some reason for believing the likelihood principle, and fresh arguments for the law can, in return, be drawn from several writings, especially those of Barnard.

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[1] P. R. Halmos and L. J. Savage, 'An Application of the Radon-Nikodym Theorem to the Theory of Sufficient Statistics', *Annals of Mathematical Statistics*, 1949, **20**, 225-41

[2] D. Bernouilli, 'The Most Probable Choice between Several Discrepant Observations and the Formation therefrom of the Most Likely Induction', translated from the *Acta Acad. Petrop.* for 1777, and published in *Biometrika*, 1961, **48**, 1-13

[3] For statement of this and other standard statistical procedures mentioned in the sequel, the reader may wish to consult M. G. Kendall and A. Stuart's *Advanced Theory of Statistics*, London, 1958, vol. ii.

[4] For an elementary presentation and some references, see L. J. Savage and others, *The Foundations of Statistical Inference*, London, 1962, pp. 15 f.

10 *Testing Hypotheses*

The law of likelihood can be applied to testing statistical hypotheses, but it could never entail that any test is appropriate, until one has characterised the purpose of a test. The law must be a tool for the foundation of a theory of testing, not its entirety. The problem of testing can be put very generally, but for present purposes, a piecemeal presentation will suffice.

Plenty of statistical tests were made centuries ago but we shall take as example the first published rationale for such a test. In 1710 Arbuthnot considered the hypothesis that there is an equal chance of an unborn child being male or female.[1] His data is information about the births in London over 82 years and derived from the public registers. In every year, males exceed females. Let us call a 'male year' one in which more M are born than F; the rest are 'female' years. Arbuthnot tests hypothesis H: 'the chance of male and of female years is equal'. He believes that if H must be rejected, so must the hypothesis of equal chance of M and of F births, but let us attend solely to his test of H against the data derived from the registers.

Arbuthnot argues that if H were true, there would be only a minute chance of getting 82 male years in a row:

'1/4836 00000000000000000000'.

Since we know there have been 82 male years he thinks the fantastically small chance of such an event, assuming H, justifies rejecting H. His conclusion prompted a most instructive discussion in the public prints, but we cannot pursue it here.

Let us suppose Arbuthnot's theory is this: reject H if the likelihood of H plus the data is small enough, that is, if the chance of getting the observed outcome, is, assuming H, very small. This theory runs into a terrible problem. For it seems that H would have to be rejected no matter what happened over 82 years. Say we observed 41 male years and 41 F, in some order, maybe MFFMF . . ., the chance of this result would be exactly the same as the chance of MMM . . ., viz. $(1/2)^{82}$. So it seems the likelihood of H on data about 82 years would be the same, no matter what was observed. So H would have to be rejected on any data, no matter what happened.

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[1] *Philosophical Transactions of the Royal Society*, 1710, 186-190.

Here it will be protested that the chance of getting 41 M, 41 F *in some order or other*, is far greater than the chance of all M. Very well. But the public registers do not report the number of M years 'in some order or other'. They give the data in one particular order, starting from 1629. Why is it right to ignore this data? Of course Arbuthnot would be right to ignore the arrangement, and right to reject H on the data of 82 M years, and wrong to do so on the data of 41 M, 41 F. But low likelihood alone is not the reason.

Somehow it seems correct to disregard the order of M and F years, so long as the outcome of any year is thought independent of that of any other. The theory of support can explain why. Arbuthnot's inference is justified not by the low likelihood of H, but by the low ratio of the likelihood of H, compared to that of other possible hypotheses. We should expect to reject an hypothesis on some statistical data if it is much worse supported by that data than some rival hypothesis. According to the law of likelihood, relative support is as relative likelihood. But how, it will be asked, does this explain our readiness to disregard the actual order of the years, and attend only to the proportion of M years? The answer is easy. Let d_1 be the data stating not only the proportion, but also the arrangement of M & F years; let d_2 be an abstract of this, stating only the proportion. Let H be Arbuthnot's hypothesis, assuming independence of the outcome of one year from the outcome of any other; let J be any other hypothesis assuming independence. Then it can be proved that

$$\frac{\text{likelihood }(H \,\&\, d_1)}{\text{likelihood }(J \,\&\, d_1)} = \frac{\text{likelihood }(H \,\&\, d_2)}{\text{likelihood }(J \,\&\, d_2)}$$

that is, the likelihood ratio is invariant under this particular abstraction. If H is better supported than J on d_1, so it is on d_2. So we can, so far as support is concerned, ignore the arrangement of years, and attend solely to the proportions. In this case of independence, proportion is what Fisher called a 'sufficient statistic': statement of the proportion is a statement of the data sufficient for statistical inference. And the H is to be rejected not because it has little likelihood, but because it has much less likelihood than some other hypothesis assuming independence. Better: it is to be rejected because there are other hypotheses very much better supported by the data than it is.

11 *Likelihood tests*

The idea that a class of rival hypotheses is necessary in testing statistical hypotheses is to be found even in Laplace, but its full importance was not realised until work by Neyman and Pearson thirty-five years ago.[1] Even today the idea is not universally accepted: Fisher explicitly denied it and he was, over two decades, the greatest of contributors to statistical science. But there are strong reasons for holding that he was wrong in this respect; the tests which he himself invented cannot be justified unless a class of possible distributions is implicitly taken into account.

At any rate, if an hypothesis is to be rejected only if other hypotheses are much better supported, our theory of support leads directly to a theory of testing hypotheses. Given a class of joint propositions consistent with some statistical data, we shall say a member h of this class is *k-rejectable* if there is some other member of the class, h', such that

$$\frac{\text{likelihood of } h}{\text{likelihood of } h'} < k$$

where k is a sort of measure of the stringency of the test. *A likelihood test with critical ratio k* will be one which rejects an hypothesis if and only if it is k-rejectable, and does not entail any joint proposition consistent with the statistical data which is not k-rejectable. A more general definition, appropriate to the first *Remark* under section 8 above, would be expressed directly in terms of the 'likelihood ratio', which may exist even if likelihoods are zero.

These tests have consequences for testing simple statistical hypotheses. Suppose that one of these, H, is under test, say the hypothesis that P(heads) = 1/2. And suppose there is some data d about the possible hypotheses and about the outcome of past trials on the set-up in question. Then H & d is essentially a joint proposition. So a likelihood test with critical ratio k will reject H if and only if there is some rival statistical hypothesis J, consistent with the data, and such that

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[1] See Laplace's argument for rejecting the hypothesis that the letters observed to lie on the table in the order C-o-n-s-t-a-n-t-i-n-o-p-l-e should have been arranged in that order by a mere fluke. Chapter 5 of the *Essaie Philosophique*. For Neyman's views, see his *Lectures and Conferences on Mathematical Statistics and Probability*, Washington, 1952, chapter 1.

$$\frac{\text{likelihood of H } \& \ d}{\text{likelihood of J } \& \ d} < k.$$

Evidently any such rejection might be tentative, because subsequent data about more extensive trials on the set-up might not justify rejection of H at level k.

If some statistical hypothesis H is to be tested by making a trial on a set-up, we may divide possible results of the trial into two classes: those which, given the data, suffice to reject H, and those which do not. The former class is called the *rejection* class. Given a set of possible statistical hypotheses, a result E will be in this class if and only if there is some rival hypothesis J such that the likelihood of J & E is at least k times that of H & E.[1]

12 *The Neyman-Pearson theory*

In 1933 Neyman and Pearson enunciated what has for many years been the received theory on testing statistical hypothesis.[2] According to them, the first consideration in testing an hypothesis is that there should be little chance of rejecting it, if it is true; secondly, there should be a good chance of rejecting it if it is false.

Let there be a given statistical hypothesis H about trials of kind K on a set-up X. Let the statistical data determine the class of possible statistical hypotheses; in addition, a trial of kind K is to be made. Then we are to form a rejection class R; if the result of the trial lies in R, H is to be rejected, otherwise not. On the original Neyman-Pearson theory, R was to be chosen so that, on H, the chance of getting a result in R is small, say ·01. This is called the *size* of the test; it used to be called the *significance level*. Recent workers have stated conditions in which tests of large size are useful, but here we shall consider only the original theory, in which small size was paramount.

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[1] Hence likelihood tests are not identical with the tests which have been called likelihood ratio tests, and which were first explained in the early paper of Neyman and Pearson, 'On the Use and Interpretation of certain Test Criteria for Purposes of Statistical Inference', *Biometrika*, 1928, **20**, 175–240 and 263–92. In likelihood tests we impose a condition on individual results and form the rejection class accordingly, but in likelihood ratio tests we place a condition directly on the rejection class. It is notable that some likelihood ratio tests which everyone agrees to be absurd (e.g. E. L. Lehmann, *Testing Statistical Hypotheses*, New York, 1959, p. 252, example 18) are not likelihood tests.

[2] J. Neyman and E. S. Pearson, 'On the Problem of the Most Efficient Tests of Statistical Hypotheses', *Phil. Trans. of the Royal Society*, A, 1933, **23**, 289–337

Secondly, for given size, the theory says that R should be chosen so that, if H is false, the chance of rejecting H should be as large as possible. The least chance of rejecting H, if it is false, is called the *power* of the test. Thus: choose small size, and then maximise power.

If there is only one rival statistical hypothesis, it is always possible to maximise the power for a given size, so there is, for given size, a test which is called *uniformly most powerful*. Moreover, in this case the UMP test will be a likelihood test, and so Neyman-Pearson and likelihood tests coincide. This result follows from what is called the Fundamental Lemma of Neyman and Pearson. (However, a best test may require a 'randomised' rejection class, so that whether or not to reject on given data is in part determined by the outcome of a trial on an auxiliary chance set-up, maybe based on a table of random numbers. This in turn leads to a more sophisticated definition of likelihood tests. An elementary example will be given in the sequel.)

But suppose that in addition to the H under test, there is not one rival hypothesis, but a pair, J′ and J″, of which one or the other must be true if H is false. For any given R, there may be a very large chance of rejecting H if J′ is true, but very little if J″ is true. There may, in fact, be no test which is *uniformly* most powerful against all the different possible rival hypotheses. When there is no uniformly most powerful test Neyman and Pearson resort to other criteria for choice of R. We shall see that these criteria lead to tests which are 'best' in the sense of Neyman and Pearson, but which are not likelihood tests. Hence there is a substantial practical difference between the two theories of testing. First, however, we must undertake a slightly more theoretical investigation of size and power.

13 *Size and power*

At first sight low size and great power seem the most desirable qualities which a test could have: low chance of rejecting what is true coupled with great chance of rejecting what is false. Not only do these qualities seem intrinsically good, but also they imply pleasant long run characteristics. If the size of a test is ·01, there is only a minute chance that in a long run of different applications of this test the proportion of mistaken rejections will much exceed 1 per cent. Yet even these desiderata are not conclusive.

Suppose that before making a trial on a set-up you were asked to choose a test of an hypothesis H, subject to the twin conditions (*a*) if H is false, you win r if you reject it and lose s if you do not, but (*b*) if H is true, you win r' if you do not reject it and lose s' if you do. You must choose your test before making a trial on the set-up; the test rejects H according to the result of this trial. Here we have what I shall call a situation of *before-trial betting:* essentially you choose a test to bet on at the stated odds. It differs from the problem of *after-trial evaluation* in which, after making a trial on a set-up, you have to decide whether or not to reject H. Now it looks as if the most sensible rejection class in the first case should be the same as that in the second, but we shall show this is not so. Size and power are the cardinal considerations only for the first case. So we shall conclude that the Neyman-Pearson theory is primarily the theory of before-trial betting, rather than after trial evaluation.

A distinction similar to that between before-trial betting and after-trial evaluation was urged last year by Dempster, though he was not arguing for quite the same conclusions as will be suggested here; other recent publications contain similar ideas, most of which seem implicit in a remark of Jeffreys's which has lain fallow for over twenty years.[1] The following pages aim at explaining some of these ideas from within the theory of support, and so serving as a test of that theory and its law of likelihood.

14 *Trivial examples*

Suppose a trial on a set-up X must have any one of three possible results; there are two rival hypotheses about this set-up as shown below; the first of these is under test. Suppose that in a before-trial betting situation you must choose a test of size ·01, to be based on

	$P(E_1)$	$P(E_2)$	$P(E_3)$
H	·01	·04	·95
J	0	·05	·95

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[1] A. P. Dempster, 'On the Difficulties Inherent in Fisher's Fiducial Argument', read at the meeting of the International Statistical Institute in Ottawa, 1963; H. Jeffreys, *Theory of Probability*, Oxford, 1939, p. 327; F. J. Anscombe, 'Sequential Medical Trials', *Journal of the American Statistical Association*, 1963, **58**, 365-383.

the result of a single trial on this set-up. You are subject only to condition (*b*) stated in the preceding section: if H is true you win 1 unit if you do not reject it but lose 100 if you do. Then any test of size ·01 is as good as any other. One such test is the test which rejects H if and only if event E_1 occurs. The odds are 100 to 1 against mistakenly rejecting H if it is true. But now turn to after-trial evaluation. If E_1 does actually occur, then J has been proven false, and so H must be correct. Hence for after-trial evaluation it would be absurd to use a test which rejects H if E_1 occurs. So tests equally good for before-trial betting need not be equally good for after-trial evaluation.

For another equally banal example, trials on set-up Y must have one of four possible results; once again there are two possible hypotheses; the one called H is under test. This time, in a situation of before trial betting, both conditions (*a*) and (*b*) shall be operative, so we shall be concerned with both size and power. Consider a test R which rejects H just if E_3 occurs, and also a test S, which rejects H if and only if E_1 or E_2 occurs. Both tests have size ·01, but the power of R is ·97 while that of S is a mere ·02. Thus there is a much better chance of rejecting H, if false, by use of R than by use of S, and so R is the better test for before-trial betting. But if E_1 does occur, there is conclusive proof that H is false, and so on this occasion the verdict of S is to be preferred to R. Once again, before and after trial considerations need not coincide.

	$P(E_1)$	$P(E_2)$	$P(E_3)$	$P(E_4)$
H	0	0·1	·01	·98
J	·01	·01	·97	·01

These examples suffice to show that size and power are not automatically the most important characteristics for after-trial evaluation. They do not, however, show anything about optimum Neyman-Pearson tests. Although R is, in Neyman-Pearson terms, better than S, it is not the optimum test for test testing H against J. In fact the Fundamental Lemma guarantees that the best test is always a likelihood test, and likelihood tests are, I contend, the tests for after-trial evaluation. Hence in the two examples just given the UMP Neyman-Pearson test will be suitable both for before-trial betting and for after-trial evaluation. This is not because being uniformly most powerful is in itself a desideratum for after-trial evaluation, but because it entails a desideratum. But now we shall prove that when there is no uniformly most powerful test, the best Neyman-Pearson test is not necessarily suitable for after-trial evaluation.

15 *Unbiased tests*

Let H and J consist of several simple statistical hypotheses, and suppose H is being tested against rival J. To some writers it has seemed odd to consider tests in which the chance of rejecting H, if it is false, should be less than the chance of rejecting it if it is true. Such a test has been called *biased;* Neyman and Pearson and most subsequent workers hold that unbiased tests are to be preferred. It often happens that even if no test is uniformly most powerful, there is a test which is uniformly more powerful than all other unbiased tests. When it exists it is called the *uniformly most powerful unbiased* test, and is held optimum for this situation. The following example proves UMPU tests need not be likelihood tests; it also proves that however desirable be a UMPU test for before-trial betting, it need not be suitable for after-trial evaluation.

A trial on a set-up Z may have any one of 101 possible results, labelled 0 to 100. According to simple hypothesis H under test, the chance of getting the result numbered 0 is exactly ·90, and of every other result, is ·001. The rival hypothesis J consists of 100 simple hypotheses $J_1, \ldots J_{100}$. According to J_i, the chance of getting 0 is ·91, of getting i is ·09, and of getting $j \neq i$ is zero. Thus the hypotheses are as sketched below.

	P(0)	P(i)	P(j) for $j \neq i$
H	·90	·001	·001
J_i	·91	·09	0

for $0 < i < 100$, $0 < j < 100$.

Intuitively speaking, if event 3 occurs on a trial, every member of J except J_3 is refuted; and if H is true something has happened which happens very rarely, only one time in a thousand, while if J_3 is true, what has happened is something which happens rather often, nearly one time in ten. So if an event $i > 0$ occurs, there is some indication that J is true; on the other hand, if 0 occurs, there is no reason to choose between H and J. The likelihood test of size ·1 accords with this intuition; it rejects H if and only if 0 does not occur on the trial which is to be made.

In this situation no test is uniformly most powerful, but there is a UMPU test. It is a ‘ mixed ’ test: let there be some entirely independent set-up, to be called the randomiser, such that a trial on this set-up yields one of two results, A and B, with $P(A) = 1/9$. The UMPU test rejects H if and only if the result of the next trial on Z is 0, and the randomiser has result A. The size of this test is ·1, just as for the likelihood test, but whereas the likelihood test has a power of only ·09, the UMPU test has a power rather greater than ·1.

No better contradiction could be desired. The two tests could not agree on rejecting H; one rejects only if 0 does not occur, the other, only if 0 does occur and the randomiser has result A. For before-trial betting the UMPU test must be preferred because of its greater power, but it would be foolish for after-trial evaluation, for it rejects H just if there is no reason to discriminate H from J. If the Neyman-Pearson theory were conceived as suitable for after-trial evaluation, we should in fact have a kind of paradox. For the optimum Neyman-Pearson test is the UMPU test. But if the result 3 occurs, the only possible hypotheses are H and J_3. And the best Neyman-Pearson test of size ·1, when H is being tested against J_3 alone, will certainly reject H for J_3, and thus will contradict the other best Neyman-Pearson test. But there is no paradox. The Neyman-Pearson theory is fit for before-trial betting, not for after-trial evaluation.

The example is interesting in another connection. The size of the likelihood test is greater than its power. You could get a test with as good a size and better power simply by using a table of random numbers and not conducting a trial on Z at all. You reject H if you select some digit, 9 say, from the random number table; the size and power of this test are both ·1. Hence for before-trial betting the likelihood test is worse than useless. But it is perfectly suitable for after-trial evaluation. Statisticians have commonly thought that being ‘ worse than useless ’ completely discredits a test,[1] but this is only true for before-trial betting.

16 *Tests and support*

The complete analysis of the domain of our two kinds of test needs explicit use of the logic of support. I have been saying that Neyman-

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[1] M. G. Kendall and A. Stuart, *Advanced Theory of Statistics*, London, 1958, vol. ii, p. 246. The example given there is a worse than useless likelihood ratio test which is not in fact a likelihood test.

Pearson tests are suitable for before-trial betting, but this is only a notion derived from that of size and power; it seems pretty hard to imagine a real-life before-trial betting situation. In fact we must distinguish four different possibilities. In each case suppose a set-up X is under investigation, and that we consider trials of kind K; hypothesis H is to be tested. Data *d* states the possible distribution of chances of outcomes on trials of kind K.

(1) *Before trial betting.* Suppose I have in mind a Neyman-Pearson test of low size and great power; before making any trial on the set-up I decide to use this test. I can compare two hypotheses: h_1, that as a result of making a trial of kind K, my test will lead me to treat H wrongly, that is, to reject it if it is true, or retain it if it is false; the alternative hypothesis, h_2, is that as a result of making a trial, my test will lead me to treat H correctly. In virtue of the low size and great power, I can assert, *On data d, h_2 is much better supported than h_1.* This follows from the law of likelihood. Because *d* supports h_2 better than h_1, and because *d* is my sole data, it may be wise to choose this test in a before-trial betting situation.

(2) *Complete after-trial evaluation.* I make a trial and observe result E, compute the relative likelihoods, and use a likelihood test to reject H. That is, I assert, *On data d, plus the fact that E occurred, H is much less well supported than some rival.* Since all I know is *d* and the occurrence of E, I reject H.

(3) *Incomplete after-trial evaluation.* I begin with the Neyman-Pearson test noted in (1). Suppose it has rejection class R, and that I make a trial and observe some result E which is a member of R. In virtue of the low size and great power, I can assert, *on data d, plus the fact that the result was some (unspecified) member of R, H is much less well supported than some rival.* But I am not automatically entitled to reject H, for I know more than that some unspecified member of R occurred; I know that E occurred; if I want to evaluate H, I must proceed as in (2). Earlier examples have shown how knowledge, that a specific member of a rejection class occurred, may alter your evaluation.

(4) *Economical after-trial evaluation.* But now suppose that, using the same Neyman-Pearson test, I had decided, before making the trial, to observe only whether or not the result of the trial lay in R, and not to note what the result was; or suppose I had decided to discard all information except that R did, or did not, occur on the trial to be made.

Then just as in (3) I can assert that on data *d*, plus occurrence of R, H is much less well supported than some rival. But in contrast to (3), *d* and the fact that R occurred complete my information. So now I can reject H because, on the data I have resolved to use, H is much less well supported than some rival.

But when can I justly decide to discard or ignore available information? It may be that observation of individual results is costly, or that their analysis is tedious; moreover, before making any trial on the set-up, I can show that in virtue of low size and great power it is a good bet that a test based merely on occurrence or non-occurrence of R will not seriously mislead. In particular, if I am to use the same test on many like hypotheses about, for instance, successive items produced by an assembly line, and if I am concerned solely to keep my average error low and to use as cheap a test as possible, I may decide simply to ignore some available information. Thus, *if accompanied by a rational initial decision to discard or ignore data, the Neyman-Pearson theory may be perfectly suitable for after-trial evaluation.*

But when so used, the Neyman-Pearson theory is not so much an alternative to the likelihood theory as a special case of it. We reject an hypothesis H on the data that R occurred because (*a*) we have decided to observe only whether or not R occurred, and (*b*) on the data that R did occur, plus *d*, H is much less well supported than some rival. Thus even though the Neyman-Pearson theory may appear to differ from the likelihood theory, one includes the other and the theory of support analyses both. The merit of the analysis in terms of support is not to discover the distinctions which have just been made—they are already to be found in the literature—but to establish their relationships within a single coherent theory. And our whole theory has required only one novel postulate, namely the law of likelihood. Even that is novel only in being stated, for it underlies even the most trivial inferences from known frequencies, to guesses about what will happen next. The preceding discussion shows it also underlies inferences in the theory of testing statistical hypotheses.

17 *Other theories*

I think these conclusions are very much in the spirit of Fisher's opinions.[1] He came increasingly to hold that likelihood is one central tool in statistical inference. I think the law of likelihood is only a spelling out, in more precise terms, of some of his intuitions. Our particular conclusions about testing agree with Fisher's. He was determined that the Neyman-Pearson theory was at most applicable to certain kinds of routine and repeated investigation, but could not serve for evaluating statistical hypotheses. He was immensely critical of Neyman's disregard for 'sufficient statistics', that is, his readiness to use tests which, from our point of view, rely on discarding or ignoring data. The theory of support and the law of likelihood may, I hope, be viewed as a philosophical analysis of ideas which Fisher perceived rather clearly, but which he expressed in only a fragmentary or polemical manner.

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[1] Fisher's views are stated, and references are given, in his *Statistical Methods and Scientific Inference*, Edinburgh, 1956.

The views on testing submitted here may also be reached from another point of view, one which is alien to Fisher's. In the subjective theory of statistical inference, pioneered by Ramsey and de Finetti, and now championed by Savage, it is supposed that a person or group of persons assigns personal rates for betting on the truth of the various hypotheses which interest him.[1] Then he makes trials on a set-up, and, as he learns of their results, is induced to change his betting rates. It can be proved that if he admits some very plausible conditions on betting, the change in his rates will be as described in a theorem Bayes proved two centuries ago. And if a person decides to reject a simple statistical hypothesis only when he will give good odds against its being true, then he must adopt what are essentially likelihood tests.

It is pleasant that the theory of support achieves a conclusion which can be urged from these very different, subjective, principles. But as far as the foundations of statistics is concerned, it must be realised that the theory of support is radically different from, and indeed very much weaker than, the subjective theory. There is no postulating of prior betting rates, no assumption about the rationality of bettors, no harassing difficulties about scales of utility. It locates the foundation of the inferences described in this paper in a single fact connecting frequency and support. In only one regard does the present theory make stronger assumptions than the subjective theory: it supposes there is a property of long run frequency which needs conceptual clarification, and it proposes four postulates by way of defining that property. Most subjectivists think they can get on without such a property; de

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[1] The best recent exposition is in L. J. Savage, *The Foundations of Statistics*, New York, 1954.

Finetti explicitly denies that sense can be made of 'long run frequency' or what I call chance. On the contrary, I urge that our four postulates make sense of it.

18 *Summary*

The law of likelihood is a proposed explication of a fundamental fact about long run frequency: a fact taken for granted even when we guess what will happen next on the basis of data about frequencies. This fact does not seem deducible from any more fundamental facts except, perhaps, ones which differ from it only in generality.

Kolmogoroff's axioms plus the law of likelihood may be regarded as part of the postulational definition of chance, or frequency in the long run. The assumed underlying logic is the usual propositional and functional calculus, plus Koopman's logic of support. The postulates can be proved consistent. They are not complete under the intended interpretation, although it is unclear what a completeness proof in this domain would be like. But at least a great many probably true propositions involving chance and support can be deduced. This theory seems at least part of the foundations of statistics.

No theory relying solely on support can entail the correctness of any theory of testing. But given some agreed desiderata for tests, the theory of support can be used to prove that some tests have these desiderata. Within the theory of support the domains of the likelihood and the Neyman-Pearson theories of testing have been explained; the conclusions reached follow inevitably from the most elementary analysis of the notion of support. However the present paper is intended primarily as a contribution to the definition of that physical property, chance, or long run frequency; it is only a token of the correctness of the postulates that they provide for a comprehensive analysis of theories of testing.

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THE INFERENCE OF FUNCTION FROM STRUCTURE IN FOSSILS*[1]

M. J. S. RUDWICK

I

THE concept of adaptation stands at the centre of the modern debate on the mechanisms of evolutionary change. The theory at present dominant among biologists, which is generally termed ' Synthetic ' by its proponents and ' Neo-Darwinian ' by its critics, stresses the ubiquity of natural selection as the prime causal factor behind the apparently ' directive ' element in evolutionary change. Adaptation is considered to be as ubiquitous a feature as natural selection; for it is the ' pressure ' of selection that evokes the first appearance, directs the development and even enforces the extinction of the features in which adaptation is expressed. (Inadaptive ' genetic drift ', postulated principally on the basis of mathematical models, occupies a relatively minor position in the theory.)

Standing against this Synthetic theory, and generally lacking both its brilliant advocacy and its present massive support, is a diverse group of theories which however have one important feature in common: they attribute a significant role to inadaptive or non-adaptive structures. This applies, for example, on Schindewolf's ' Typostrophic ' theory, to the ' ground-plan ' of the first members of a new major taxon; or, on most varieties of the theory of Orthogenesis, to the ' bizarre ' features of the last surviving members of an evolutionary trend. ' Unorthodox ' theories such as these are often attacked intemperately on the grounds that they are ' unscientific ', ' metaphysical ', or even ' mystical '.[2] This is certainly unjust to many (though not all) who

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[1] This article expands and develops a brief introductory section to my paper on ' The feeding mechanism of the Permian brachiopod *Prorichthofenia* ' (*Palaeontology*, 1961, 3, 450-471), which describes a functional reconstruction of a highly aberrant extinct organism.

[2] See, for example, the frequent pejorative use of ' metaphysical ' in G. G. Simpson, *The Major Features of Evolution*, New York, 1953, pp. 28, 63, 73, 134, etc.

have propounded these theories: internal orthogenetic factors, typostrophes, etc., have often (though not always) been postulated as strictly scientific explanations.

As Dr Marjorie Grene has shown in her lucid analysis of the disagreement between Schindewolf and Simpson, this whole debate is 'an illuminating paradigm of scientific controversy'.[1] These two palaeontologists see the same evidence from widely different viewpoints, and are therefore separated by disagreement on several different planes. Each of their respective ways of looking at the evolutionary record 'constitutes, for its proponent, a closed interpretive system *in* which he sees the facts. In such a case the objections of one to the arguments of the other are, from the other point of view, irrelevant'.[2] The Synthetic theory and the theories standing in opposition to it do indeed show many of the marks of closed all-explanatory systems. This is reinforced by the personal attitudes of their supporters: the Synthetic theory shows a disturbing tendency to become an illiberal and intolerant orthodoxy, which is matched by the tendency of its opponents to regard themselves as an embattled minority. This situation has obscured the possibility of discovering criteria that might be accepted by both sides as being valid tests.

An example of such a test is to determine whether or not a series of fossil species existed in the order predicted by an orthogenetic interpretation of their evolution. Thus Simpson has justly argued that the many orthogenetic interpretations of the evolution of the horses and the sabre-toothed 'tigers' are invalid, simply because the fossil evidence shows that the evolution was not a single directional trend at all, or because the species occur in the wrong order.[3] But in fact this is a one-way test; it involves a 'risky prediction' on one side but not on the other. An orthogenetic explanation involves a prediction of the order of occurrence of certain fossils; and this prediction may be (in some instances has been) proved incorrect. But there is no comparable prediction in a Synthetic explanation. If the fossils do not form a trend, that is easily explained; but if they do, that too can be explained in terms of selection and adaptation. Even if there was a genuine trend, culminating in structures that some would judge to have been non-adaptive and therefore favouring an orthogenetic explanation, the Synthetic theory is not disturbed at all. For its supporters can always argue that these structures, however 'bizarre', *might* have had an adaptive value.

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[1] M. Grene, 'Two Evolutionary Theories', this *Journal*, 1958, **9**, 110-127, 185-193

[2] Ibid. p. 185

[3] Simpson, op. cit. pp. 259-265, 269

It is this asymmetrical relation between the theories that I want to examine more closely in this article.

2

Adaptation is a critical issue between the Synthetic theory and its rivals. Are there, or are there not, important features of the evolutionary process which cannot be explained in terms of adaptation under the influence of selection? Is the 'ground-plan' of a new major taxon (for example the tetrapod vertebrates) initially no more than a slight adaptive modification of that of its immediate ancestors; or is it genuinely novel, and not in itself adaptive, however many adaptations it may subsequently give rise to? Are the first rudiments of a new organ (for example the rudimentary horns of the earliest titanotheres) themselves of adaptive value, however slight; or are they initially no more than fortuitous inadaptive variants, even though, enlarging under the influence of some internal (perhaps genetic) factor, they may subsequently become adaptively significant? Does the extreme terminal development of such an organ (for example the antlers of the 'Irish elk') reflect the ever-increasing perfection of its adaptation under the continuing pressure of selection; or does it represent a trend carried to a point at which the organ has ceased to be adaptively valuable and become disadvantageous to the organism that possesses it? Does the extinction of such an organism reflect its inability to modify a highly adaptive but narrowly specialised mode of life in the face of changing circumstances; or is it the inevitable result of a trend that has equipped the organism with a feature which has become too disadvantageous to permit survival?

With such questions as these, the argument between the Synthetic theory and its rivals can be seen to depend critically on the recognition of the adaptive value—or lack of it—of the structural features of particular organisms. If this were all, the argument could be settled—at least in principle—by determining, by observation and experiment, whether or not these features perform identifiable functions in the economy of the organisms.

But the problem is more difficult. For the organisms whose adaptive status we wish to evaluate can only be recognised as such *in retrospect*. The first members of a new major taxon, if discovered today, would not be recognised for what they were. If (on the Synthetic theory) they differed only slightly from their immediate ancestors, they would be placed taxonomically in the same major taxon, and not in a new one of their own; whereas if (on the Typostrophic theory) they showed a radically novel ground-plan, they would merely be assigned to a new 'minor phylum' (as for example the Pogonophora have been in recent years), and their adaptive status would be of no great significance. On either theory it is only in retrospect, in view of the abundance and diversity of the adaptive forms that subsequently arose from them, that the earliest members of a new major group can be recognised and their special significance appreciated. Similarly, the earliest rudiments of a new organ that gradually develops into great prominence can only be recognised in retrospect, after the organ has developed: such rudiments, if discovered in an existing organism, would be regarded as minor variant features of no great importance. Again, continued directional change in such an organ can only be recognised over a long period of geological time, and not in the evolutionary 'instant' in which we can study living organisms. Finally, it is only in retrospect that the extreme development of a structural feature can be seen to have been connected—by whatever cause—with the extinction of the form that possessed it.

Since the critical evidence can only be recognised in retrospect, the alleged ubiquity (or near-ubiquity) of adaptation can only be tested against the alleged existence of important non-adaptive features by referring to the evidence of palaeontology. It is on fossil material, or not at all, that we can hope to test the validity of this aspect of the Synthetic theory and its alternatives. (The reality and significance of this situation has been obscured by the tendency of many palaeontologists, with more imagination than logic, to use the present tense to describe phenomena that in the nature of the case belong to the unobservable geological past.)

That the critical evidence is limited to fossil material introduces problems which, although they are strictly technical in origin, have important theoretical implications. The fossil record is highly imperfect, not only as a record of the course of evolution, but also as a record of the adaptations of individual organisms. Unavoidably, the range of specific adaptations that we can hope to detect in fossil material is therefore limited to those that are reflected in preservable features of the morphology and environment of the organism. The evidence of environmental factors may be extremely scanty, and it is often only possible to reconstruct the external environment in the most general terms. Factors of the environment, physical or biotic, which might be essential to the understanding of an adaptation, may have left no trace. Moreover, the preservation of the organism itself is also invariably imperfect: whole organs or parts of organs, which might be essential to the evaluation of the adaptive status of the organism as a whole, may have left no trace. However, favourable modes of preservation may record at least the skeletal structures in extremely fine detail, and occasionally other parts of the anatomy also; and in some taxa an intimate relation between skeletal and non-skeletal parts may allow much of the unpreserved (non-skeletal) anatomy to be reconstructed with considerable confidence from indirect traces in the skeletal anatomy (e.g. muscle insertions and cranial anatomy in fossil vertebrates). Such reconstructions depend essentially on the detection of homologies between the fossil and some related living organism; if the fossil belongs to a wholly extinct group (e.g. graptolites) the problem is far more difficult.

But even with the most favourable circumstances of preservation, we can only re-create the fossil organism, with a lesser or greater degree of confidence, into a state in which it resembles a pickled specimen of some present-day organism. Anatomically it may be more or less complete, but it is isolated from most features of its original environment, and of course it lacks the ongoing activities of life. The functions —if any—of its various parts and organs therefore cannot be determined by simple observation and experiment; we cannot (as we can, in principle, with any living organism) simply observe what effects an organ in fact achieves in natural or experimentally contrived situations.

The same difficulty often arises in practice even in the study of living organisms. For example, direct observation of the function may be impossible except by dissection of a kind that modifies or even inhibits the function; or an organism may only be known from dead specimens (a common situation, for example, in the study of deep-water marine animals). But though reasons such as these may sometimes hamper the study of the functional morphology of living organisms, the impossibility of a direct study of function becomes in palaeontology not merely incidental but normative. It is this that has led some palaeontologists to dismiss all inferences of function from structure in fossils as purely speculative, unscientific, and useless. Many such inferences have indeed been advanced without valid arguments to support them, but clearly this does not warrant a wholesale rejection of them all, without an examination of the basis on which they are or can be made.

3

When the inference of function from structure is discussed in palaeontological works, it is commonly said that the fundamental methods of reasoning involve comparisons—either homological or analogical—with living organisms. These however are pseudo-methods, which conceal the real nature of such inferences.

Suppose that we have discovered the fossil remains of an extinct organism, and we wish to determine the adaptive significance (if any) of its distinctive features. Suppose that what we have found is in fact a pterodactyl.[1] We notice its long slender forelimbs, and the traces of a thin membrane stretched behind them. What is the adaptive significance of these forelimbs? We notice their resemblance to the wings of birds (though there is no trace of feathers) and to the wings of bats (though the skeletal framework of the membrane is significantly different). It is clear that the pterodactyl is a tetrapod vertebrate, so that we are justified in regarding the comparison between its forelimbs and the forelimbs of birds and bats as being—at least in a general way—a *homological* comparison. Yet if we provisionally identify the fossil structure as a 'wing', we are obviously not doing so by virtue of its (imperfect) homology with the wings of birds and bats. If homology were the real criterion, we ought to be comparing it rather with the forelimbs of some more closely related living organism, such as a lizard; but this would lead us to identify it as a crawling organ, which we would rightly reject as highly improbable.

Why then do we call it a 'wing'? We do so, of course, because we recognise its *analogical* similarity to the wings of birds and bats. Even if we knew of no birds or bats, we might still see its similarity to the wings of, say, insects, with which it has not the slightest homological relation. Birds, bats, and insects, as we know from direct observation, use their wings for flying; the pterodactyl forelimbs have a certain structural resemblance to these organs-for-flying; therefore, we may say, the pterodactyl also probably used its forelimbs for flying.

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[1] I use this example only because the pterodactyl is a fossil that is fairly generally known: my methodological argument does not depend on the validity of this particular functional interpretation of the pterodactyl.

Yet if we stopped there, at a simple analogical comparison, we would in fact be seriously wrong. For a further study of the pterodactyl anatomy shows that it could not have possessed the powerful flight muscles which would be necessary for a full analogy with the flight of birds, bats, and insects. The analogy, like the homology, is imperfect. But this imperfection gives us a glimpse of the real method of inference which underlies reasoning that appears to be based on homological or analogical comparisons. For our final conclusion that the pterodactyl wing was not an organ for powered flight but probably an organ for gliding is based only incidentally on any comparison with living organisms.

The initial clue about its possible function may have been suggested by the observation of roughly similar structures in certain living organisms; but the development and testing of the interpretation depend fundamentally on applying a criterion of *mechanical fitness*. From our knowledge of natural and artificial aerofoils, and of the structural requirements of their successful operation, we conclude that the pterodactyl forelimb would have been physically *capable* of functioning as an aerofoil. From our knowledge of the energy requirements for powered flight and of the energy output of vertebrate muscle, we conclude that it would *not* have been capable of functioning as a flapping wing for powered flight. Given enough knowledge of the physical requirements of flight-mechanisms, we could reach the same conclusions, even if there were no homologous or analogous structures in any living organisms. Even though no living organism utilises, for example, the operational principles of the helicopter,[1] an extinct organism that did so could, if discovered, be recognised for what it was. All we need, ideally, is a knowledge of the operational principles involved in all actual or conceivable flight mechanisms possible in this universe. Consequently the range of our functional inferences about fossils is limited not by the range of adaptations that happen to be possessed by organisms at present alive, but by the range of our understanding of the problems of engineering.[2]

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[1] The nearest approach is in the hovering flight of humming birds: cf. R. H. J. Brown, 'The Flight of Birds,' *Biol. Rev.*, 1963, 38, 460–489.

[2] In view of the basic limitations of working with fossil material, the mechanisms and adaptations I have chiefly in mind throughout this discussion are those that are 'mechanical' in the narrower sense; but the same reasoning would apply equally, for example, to biochemical and physiological mechanisms, provided only that they were expressed in the preservable portions of the morphology and environment of the organism.

An analysis of fossil structures in the light of operational principles is thus, I believe, fundamental and unavoidable in any inference about their possible functions or adaptive significance. This involves the limited ' teleology ' that is inherent in any description of a machine as a machine.[1] However much the word ' teleology ' may be used as a term of abuse by polemical biologists—or philosophers—it is well known that the language of modern biologists (and especially our informal unpublished language) abounds in words such as ' contrivances ', ' mechanisms ', and ' devices ', which reveal our common conception of the organism as being (at least from this point of view) a complex of living machinery. Machines can only be described for what they are by referring to the way in which their *design* enables them to *function* for their intended *purpose.* Such purposive language cannot be avoided for some purely physical description of the machine, and no engineer would be so foolish as to attempt to avoid it. Organisms, when considered in their aspect as living machinery, must be similarly described: purposive language is unavoidable, and should not be concealed by a ' pseudo-substitution ' of apparently non-teleological terms.[2] Of course, the ' purpose ' implied in this is no more than the existence and survival of the individual organism, which from this point of view must be regarded as an end in itself; such a ' purpose ' need not be evaded or concealed for fear of implying a Paleyan metaphysic.

This fear probably accounts in part for the surprising neglect of functional inferences in palaeontology. Although purposive categories are equally necessary in any functional study of living organisms, it is easier to conceal them by ' pseudo-substitutions ' of various kinds; but with fossil material, where functions cannot be directly observed, an explicitly purposive analysis of the machine-like character of the organism is less easily evaded. So deep-rooted is the fear of following in Paley's footsteps, that even this limited teleology is widely regarded as ' unscientific '.

4

I have argued that the detection of any adaptation in a fossil organism must be based on a perception of the machine-like character of its parts, and on an appreciation of their mechanical fitness to perform some function in the presumed interest of the organism. This method of inference must now be examined in more detail.

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[1] Cf. M. Polanyi, *Personal Knowledge*, London, 1958, pp. 328-331

[2] Ibid., pp. 359-361

With a problematic fossil structure before us, the first requirement is to perceive its possible fitness for some function that might conceivably have been relevant to the life of the organism. Often this first clue will be suggested by the recollection of a living organism with a somewhat similar structure, the function of which is already known. Alternatively we may recollect some purely artificial man-made device, which solves a functional problem similar to one that the fossil organism might possibly have encountered. But in either case we shall not perceive the similarity unless we are deliberately looking for a purposive, i.e. functional, explanation of the feature we are studying. If our primary purpose is the construction of phylogenies, or taxonomic classification, we shall be blind to the clues that might lead to successful functional inferences. Only by an explicitly functional approach shall we be able to perceive that the fossil structure on the one hand, and the living or artificial structure on the other, may possibly embody the same operational principles—despite even great differences in their physico-chemical constitution (as between the wings of a pterodactyl and those of a glider).

This perception, however, is no more than a clue. The similarity perceived might be purely fortuitous, rather than the reflection of a common function. The clue must therefore be tested. The origin of the clue, in the perception of an analogy with a living or artificial mechanism, now becomes irrelevant: only the function that it has suggested to us is important. For we can only test our tentative interpretation by assessing the mechanical fitness of the fossil structure as a potential agent of this particular function. Such an assessment demands a standard against which we can judge the potential efficacy of the structure; and this must be derived from a consideration of the function itself. How can this function possibly be fulfilled? What operational principles are involved? What basic characteristics must any such mechanism possess? What, in the most general terms, is the specification to which any structure must conform, if it is to fulfil this function effectively? Such questions lead us to a generalised structural specification for the function (for example, a general specification for aerofoils for gliding). But this will probably be too vague and imprecise to be of any great use. The specification can be made more rigorous by taking into account the limitations imposed by the properties of the materials in which it would have to be embodied—if at all—in the fossil structure (for example, in a pterodactyl, the anatomical relation between the forelimb skeleton and the wing-membrane, and the finite strength of these materials, would presumably limit the possible range of ' design ' of the aerofoil). Such a specification then describes *the structure that would be capable of fulfilling the function with the maximal efficiency attainable under the limitations imposed by the nature of the materials.* Notwithstanding a rather different usage in other contexts, I have proposed that such a specification should be termed a *paradigm* for the function.[1]

In order to determine the optimal conditions for the performance of the function, theoretical considerations may need to be supplemented by experiments with actual models, unless the physico-chemical situation is very simple. Clearly many different paradigms, of different degrees of comprehensiveness, can be specified for any given function; this is important, for with fossil material many significant properties and parameters may be known inaccurately or not at all, so that a highly rigorous paradigm may be unusable.

Our clue that the fossil structure might have been an adaptation to a particular function can now be tested by comparing the structure itself with the paradigm. The paradigm is in effect a structural prediction, which by this comparison we expose to a ' risky ' test. For the degree of approximation between any paradigm and an observed fossil structure is a measure of the degree of efficiency with which the structure would have been physically *capable* of fulfilling the function. Moreover, by transforming rival possible functions into their respective paradigms, rival structural predictions can be made; and these can be judged against one another by direct comparison with the observed structure of the fossil. This comparison shows which function, of all that have been suggested, could have been fulfilled most effectively by the fossil structure. Our confidence in the result of this test will be cumulatively strengthened if we can show that it concurs with the results of similar ' risky ' tests on other parts of the organism; if, in other words, we can gradually build up an intelligible reconstruction of the way in which the various organs interacted in the service of the whole organism to achieve a possible mode of life.

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[1] Rudwick, op. cit., p. 450. The simpler word ' model ' cannot be used in this context, since it is needed to describe actual models (in the everyday sense) of reconstructed fossil organisms; in any case the word ' model ' would fail to convey the element of maximal functional efficiency.

But the ease and confidence with which we can interpret any structural feature will be proportional to its degree of resemblance to the paradigm with which we have compared it—and hence proportional to the quality of its putative adaptation. The more highly adapted it in fact was, the more closely and unambiguously will it resemble the appropriate paradigm; and hence the more readily shall we perceive the possibility of this interpretation and confirm its validity by detailed comparison. If it shows a high degree of resemblance to a particular paradigm, we may feel justified in saying that it was, very probably, an efficient adaptation to that function. If its resemblance to the paradigm is only moderately close, on the other hand, it is more likely to bear an equal resemblance to the paradigm of some other function, and we may be unable to judge between them. This will be more difficult to interpret. It might mean that the structure was rather poorly adapted to one function, and had a purely fortuitous resemblance to the paradigm of another. Alternatively, it might mean that the structure was rather poorly adapted to two different functions independently. Or again, it might mean that it had a dual function, but represented the optimal compromise between two partially incompatible specifications, and was therefore 'highly adapted' to that combination of functions.

But if our structure shows only a negligible resemblance to any of the paradigms with which we compare it, all we can conclude is that it could not possibly have been an effective adaptation for any of these particular functions. We are not justified in saying that it was therefore inadaptive or non-adaptive, because of course we can never exclude the possibility that it might have been highly adapted to some other function that we have failed to consider. We may have failed to perceive its similarity to the appropriate paradigm: perhaps we are not familiar with any living or artificial structures that embody the same operational principles; or perhaps the structure as fossilised is too small and incomplete a part of the whole functional mechanism with which the paradigm would ideally be compared. But in any case we can never justly conclude that the structure had no adaptive value.

Thus there is an asymmetry between the assertion that a particular fossil structure had a definite adaptive value, and the assertion that it had no adaptive value. There can be positive and cumulative evidence for the former (based on substantiation of 'risky predictions'), but it is logically impossible ever to demonstrate the latter.

5

A comparison between a fossil structure and the paradigm for a postulated function shows whether the structure would have been physically capable of fulfilling that function. It cannot however establish that in fact it did fulfil that function. Even if we can show that the forelimbs of a pterodactyl were so constructed that they could have functioned as a highly effective gliding mechanism, it is still conceivable that the animal habitually kept them folded away, and that they were inadaptive or even (perhaps by getting in the way of its other movements) actually non-adaptive. However improbable this may seem, it cannot logically be dismissed altogether. If in practice we do dismiss it, and feel no qualms in doing so, it can only be because we are applying a criterion of intelligibility. Either we accept—however provisionally—an explanation that is rationally satisfying because it 'makes sense' of the phenomena before us as we understand them at present; or, in rejecting that explanation, we shall leave ourselves with no explanation at all. To proceed from 'This structure could have fulfilled this function' to 'This structure did fulfil this function' involves a logical jump; our readiness in practice to make that jump is grounded not only in an analogy with living organisms, whose functional mechanisms we can observe in operation, but also in a conviction that extinct organisms, like those at present alive, followed their various modes of life by the use of machine-like adaptations based on rationally intelligible principles. An appreciation of the organism in these terms cannot be derived from the detailed study of its fossil remains, for it is the necessary condition for any such study. Without such an appreciation we shall fail even to perceive the evidence on which any functional reconstruction must be based.

But this is an appreciation of the organism considered as 'an end in itself'. Functional inference involves an analysis of adaptation only as a static phenomenon. The perception of the machine-like character of the parts of the organism is logically independent of the origin of the structures concerned. Theories of their causal origin (e.g. by natural selection or by orthogenesis) and theories of their temporal origin (i.e. by a particular ontogenetic and phylogenetic sequence) are strictly irrelevant to the detection and confirmation of the adaptation itself. The functional reconstruction of fossils is thus logically unrelated to any and all evolutionary theories.

This probably explains why the earliest effective work on the functional morphology of fossils, by Georges Cuvier and his disciples, suffered a spectacular eclipse after the general acceptance of evolutionary theory. For Cuvier's opposition to evolutionary theories arose fundamentally from his interest in the interacting functional mechanisms displayed by the organs of the body; and this is most readily allied to a static, i.e. non-evolutionary, conception of the history of the organism. It is easier to imagine that a complex of delicately co-ordinated mechanisms was 'constructed' once and for all, and that once constructed it would have to remain stable, than it is to imagine an imperceptibly gradual modification, and ultimate transformation, of such mechanisms.[1]

But although logically unrelated by any evolutionary theory, the functional reconstruction of fossils would in practice be encouraged by any theory that stressed the machine-like character of organisms. Conversely, it would be impeded by any theory which tended to devalue the machine-like character of organisms. Thus for example even the Synthetic theory, despite its great *verbal* emphasis on function, tends to dissolve genuine adaptation[2] into the non-morphological concepts of gene-pool, genetical 'fitness', adaptive zone, etc. Similarly the Typostrophic theory of Schindewolf, despite its great verbal emphasis on structure, tends to dissolve genuine adaptation into the non-functional concepts of type, Bauplan, etc.

In any case, however, the evaluation of theories such as these depends in part on critical tests which are unavoidably *palaeontological*, and which involve the assessment of the adaptedness, or non-adaptedness, of particular extinct organisms. The question is: are there, or are there not, important non-adaptive phenomena in the evolutionary process? The answer, insofar as it relates to these palaeontological tests, must bear the same asymmetry that we have found in the evaluation of the critical evidence. With a certain critical fossil organism before us, we may argue whether its distinctive features were or were not adaptive. But we cannot reach any conclusion about these features except by testing them as possible embodiments of conceivable operational principles. We may then be able to demonstrate that they were probably adaptations of high efficiency for particular functions; and by doing so we shall add cumulative weight to the Synthetic theory (or any other theory that stresses the ubiquity of genuine adaptation). Yet if we are unable to do so, our failure will not add corresponding weight to some other theory, for it can always be said that the features may prove to have been adaptive, if only we can think of the right function. For there is no positive criterion by which non-adaptedness can be recognised and demonstrated. At least in this respect, therefore, the Synthetic theory would seem to be as unfalsifiable as its rivals are unverifiable.

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[1] See, for example, W. Coleman, 'Georges Cuvier, Biological Variation and the Fixity of Species,' *Arch. int. Hist. Sci.*, 1962, pp. 315-331.

[2] i.e. the demonstrable relations between the structures of organisms and their functions, as opposed to the so-called adaptation of 'increase in fitness' which is attributed to 'genetical Selection': cf. M. Grene, 'Statistics and selection,' this *Journal*, 1961, 12, 25-42.

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REASON AND EXPERIMENT IN RELATION TO THE SPECIAL RELATIVITY THEORY *

HERBERT DINGLE

1 *Introduction*

A SHORT time ago I published in *Nature* [1][1] an argument purporting to show that the special theory of relativity, though mathematically self-consistent, could not correctly describe the facts of nature since it imposed contradictory requirements on measuring instruments—in particular, clocks. This aroused considerable comment, to which I replied in a later issue of *Nature* [2]. The result, so far from tending towards a settled view, has been an influx of correspondence so great that *Nature*, with its limited scope and the obligation to deal equitably with all branches of science, has found it impossible to do justice to the situation, and has been forced to leave it to publications allowing of more extended treatment. The correspondence has been diverted to me, and its quantity and character show such a variety of conceptions of the meaning of the theory, and consequently such misunderstanding of the nature of my criticism of it, as to prohibit a termination of the controversy at this point. It is understandable, though no less indefensible, that there should be an intuitive resistance to any criticism of a theory which is supposed to have received overwhelming experimental confirmation and to be logically impeccable, and there is much evidence that this, rather than direct attention to the point I raised, has dictated the nature of at least most of the replies, for in fact there has been none which goes to the heart of the matter.

The essence of my argument was this. Einstein deduced, from the basic ideas of his theory, that a moving clock works slower than a stationary one. By a similar line of reasoning I deduced, from the same basic ideas, that the same moving clock works faster than the same stationary one. Hence the theory, since it entails with equal validity two incompatible conclusions, must be false. Clearly it does not

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[1] These numbers refer to the references on p. 61

matter whether the deductions from the theory are both right or both wrong. If right, the theory is discredited because they contradict one another; if wrong, because it has a false implication. The only way to save it is to show that Einstein's deduction is genuine and mine not, and this, notwithstanding that I italicised the statement of its necessity in my first letter and re-emphasised it in more than one way in my later comments, no-one has even attempted. Attention has been wholly concentrated on details which it has not been perceived are related to both deductions in completely equivalent ways.

The extent of the misunderstanding of the theory—the danger of which in present circumstances is obvious—is shown by the fact that critics of my argument are about equally divided into those who hold that Einstein's deduction is right and mine wrong, and those who hold that both are right but not contradictory. The reasons given for each contention, however, show wide divergences. Those in the former class give various reasons why Einstein's deduction must be right, and conclude that mine, since it differs therefrom, must be wrong. This, of course, presupposes that the theory, being necessarily right, cannot have false implications, which is the point at issue; and no-one who reasons in this way has thought it necessary to say *where* my deduction errs. Critics in the latter class, of whom the most distinguished is Professor Max Born [3], assume that I have made one of the elementary errors to which beginners in relativity theory are so prone (and which, in fact, I learned to avoid before many of them were born), and so blind themselves to the actual point. It is no doubt for this reason that the specific questions which I have put—which, if truly faced, would have made clear the nature of my argument—have been universally ignored. There are indeed some (including an American Press Agency) who have read my letter with so little care that they take it to be a discussion of the clock paradox!

All this compels a further effort to present the essence of the theory in terms which, taking into account the nature of the confusions thus revealed, may be expected to be as little susceptible as possible to misunderstanding. But there is a second reason why such an effort is demanded at the present time. There have lately been several attempts [17, 21, 22, 23, 24] to test Einstein's second postulate by experiment. The special relativity theory rests on two postulates and a definition, and the second postulate states that 'light is always propagated in empty space with a definite velocity c which is independent of the state of motion of the emitting body' [7]. This, unlike the theory as a whole, cannot be tested by reasoning and existing knowledge alone; it is essentially a matter for experiment, and no conclusive experiment has yet been performed. One is therefore highly desirable. If my disproof of the theory is valid, one of its three foundation principles must be faulty, for the theory follows by pure logic from those principles, and it is possible, though not necessary, that it is the second postulate that fails. The fact is, however, that almost all the tests made or proposed are not true tests of the postulate, and the belief that they are reveals again a basic misunderstanding of the nature of the theory. Such tests are technically very difficult and often involve large sums of money. They are likely to be continued, since there is no unawareness of the importance of establishing this postulate on firm experimental grounds, and the experiments so far made are believed to give conflicting results. It is therefore a matter of urgency that the nature of the postulate shall be fully understood before further time and money are spent on projects which otherwise can lead only to confusion and misconception. Properly understood, the results are not contradictory because they test different things, and none of them is a completely valid test of the second postulate.

In this situation I have attempted, in the following sections, first, to state the essence of the special relativity theory in the Minkowski form—which is that favoured by most correspondents and to which I did not refer in the earlier discussions—and to show how the contradictory implications inevitably follow; and, secondly, to consider the relation of the theory to experiment. I also take advantage of the opportunity afforded of pointing out a widespread error concerning the relation of mathematics to physics, which has effects extending beyond this particular problem and especially evident in modern cosmology.

2 *Minkowskian Mathematics and Einsteinian Physics*

A. The Minkowski Form of the Special Relativity Theory. The special relativity theory was first given in complete form by Einstein in 1905 [4]. It contained a mathematical theory concerning the relation between the co-ordinates of systems in uniform relative motion, and a physical theory of the relation of those co-ordinates to the readings of clocks and measuring rods stationary in the respective systems and adjusted in a specified way. The mathematical part was identical with that of a theory of Lorentz [5], published shortly before, by which he sought to reconcile his electromagnetic theory with apparently conflicting observations, but the physics of Einstein's theory was quite different from that of Lorentz's.

In 1908 Minkowski [6] gave a mathematical disquisition in which he re-expressed the mathematics common to the theories of Lorentz and Einstein in geometrical form, and the fact that he called his co-ordinates 'space' and 'time' should not mislead us into thinking that his paper contains anything necessarily relevant to physics: it is nothing but pure mathematics, and independent of both to Lorentz's and Einstein's physical theories. Here it is sufficient to use it as a medium for describing Einstein's theory, which then takes the form given below. Although I do not think that the description I am about to give would evoke the least opposition if presented in an orthodox textbook on the subject, it is inevitable, since it is to be used as the basis of a disproof of the theory, that each item will be scrutinised with particular care as a possible source of error. I therefore give references for the various items which justify their inclusion as true elements of the theory as it is generally understood, and I trust that these will be examined without prejudice by anyone who feels inclined to doubt the authenticity of this statement of the theory.

(a) All natural occurrences (or at least all those with which the theory is competent to deal) can be analysed into *events* (point-instants), each of which is completely specified by four co-ordinates of which three are spatial and one temporal [8, p. 10]. (Minkowski [6] wrote: 'The multiplicity of all thinkable x, y, z, t systems of values we will christen the *world*').

(b) For all co-ordinate systems in uniform relative motion, these co-ordinates are related by the Lorentz transformation [4].

(c) Between any two events there is an absolute interval, ds, which is a function of the four co-ordinate-differences of those events, and is the same for all co-ordinate systems. Restricting ourselves to one space-co-ordinate, x, and taking c to be unity, for simplicity, this interval is related to the co-ordinates, (x, t) and (ξ, τ), of any two inertial co-ordinate systems by the following equations [8, p. 17]:

$$ds^2 = dt^2 - dx^2 = d\tau^2 - d\xi^2 \qquad (1)$$

(d) In any such co-ordinate system, all stationary clocks, synchronised by light signals travelling always with velocity c, record the same time for each event, and that time is equal to the t-co-ordinate of the event in that system. For such a clock situated at the event, the time of the event is the reading of the clock at the event. For any other such clock, the time of the event is the reading of the clock at the instant of receipt of a light signal from the event, minus the time of travel of the light [7, p. 40].

> (e) If, between any two events, the clocks of one system record an interval dt and those of another system an interval $d\tau$, the ratio $dt/d\tau$ is the ratio of the rates of the clocks in the two systems. Since, by hypothesis, all clocks run uniformly, this ratio must be a constant, single-valued quantity. (Einstein's determination of the rate-ratio [4] is based on this definition, and he obtains for it a constant quantity presented as the single value of the rate-ratio).

If this statement be accepted, it follows immediately that the theory contains a contradiction. For, from (1) we have:

$$(dt/d\tau)^2 = 1-(d\xi/d\tau)^2+(dx/d\tau)^2, \tag{2}$$

and this is seen not to be single-valued but to have as many values as there are possible space-intervals, dx, $d\xi$, between the events—i.e. an infinite number of values. In Einstein's deduction of the rate-ratio, which I quoted in my letter in *Nature* [1], he chose two events on a particular clock stationary at the origin of the (ξ, τ) system—namely, the event at which that clock coincided with the clock fixed at the origin of the (x, t) system, and some later event on the former clock. For these events, $d\xi = 0$, so (2) becomes

$$(dt/d\tau)^2 = 1+(dx/d\tau)^2>1, \tag{3}$$

and it is readily seen from the Lorentz transformation that this quantity has the value $(1-v^2/c^2)^{-1}$ at which he arrived. This requires the τ (' moving ') clocks to work slower than the t (' stationary ') clocks. In my deduction I chose two events on a particular clock stationary at the origin of the (x, t) system—namely, the event at which that clock coincided with the clock fixed at the origin of the (ξ, τ) system, and some later event on the former clock. For these events, $dx = 0$, so (2) becomes

$$(dt/d\tau)^2 = 1-(d\xi/d\tau)^2<1, \tag{4}$$

and it is readily seen from the Lorentz transformation that this quantity has the value $(1-v^2/c^2)$ at which I arrived. This requires the τ (' moving ') clocks to work faster than the t (' stationary ') clocks.

I do not think it is possible, with the argument in this form, to claim (as Born [3] did in relation to the original form) that I ' exchange the co-ordinate systems '. I do nothing different from Einstein except that I choose a different pair of events for determining the ratio of the clock-rates. Hence the *only* way of saving the theory is to show that a pair of events on a clock stationary in one, and not the other, of the co-ordinate systems in question is uniquely valid for calculating the ratio of the clock-rates; and this, of course, is impossible because it is an essential part of Einstein's (unlike Lorentz's) theory that all inertial co-ordinate systems are equivalent in every respect. It cannot be held that this general process is not a legitimate one for finding the ratio of the clock-rates, because it is the one which Einstein used, and I simply follow his lead. It cannot be maintained that, on the theory, this ratio is in fact a many-valued quantity, because the value which Einstein claims for it is itself constant and single-valued. I therefore assert that this proves conclusively that the special relativity theory, though mathematically self-consistent, requires clocks to behave in an impossible manner, for one set of synchronised clocks must concomitantly go both faster and slower than another set, and this is impossible.

To complete this aspect of the discussion I must add that it is not a sufficient interpretation of the contradictory results expressed by (3) and (4) to say that each represents the view of the matter taken by an observer in a particular co-ordinate system, and that therefore these results are no more contradictory than the observations of two persons of equal size, at a distance from one another, that to each the other appears smaller than himself. That, of course is not a contradiction, but Einstein's theory asserts more than that: it asserts that (3), and not (4), represents the *objective ratio* of the rates of the clocks stationary in the respective systems. This is shown clearly enough by the fact that he immediately *deduced* from (3) that a clock at the equator goes more slowly than a similar clock at one of the poles. It does not matter for our purpose whether this particular deduction is permissible or not; it is sufficient that it shows unmistakably the sense in which Einstein understood the ratio given in (3), and a glance at almost any textbook on the subject will show that this is the sense in which the ratio has been understood ever since. No one, to my knowledge, has ever suggested that it is equally true to say that a polar clock goes more slowly than an equatorial one, and, indeed, it has recently even been claimed that Einstein's non-reciprocal result has been confirmed by experiment [9]. Moreover, unless (3) is held to rule out (4), it may be proved—the proof is withheld for want of space—that the special relativity theory cannot fulfil its purpose of justifying the classical electromagnetic equations. While, therefore, it is quite true that the theory requires observers in relative motion to draw opposite conclusions regarding the relative rates of clocks, it does not regard those conclusions as having equal objective validity in relation to observation, and it would become ineffective if it did.

I must now consider an argument advanced by the more mathematically minded workers in the subject, expressed by Born [3] in the statement: 'The simple fact that all relations between space co-ordinates and time expressed by the Lorentz transformation can be represented geometrically by Minkowski diagrams should suffice to show that there can be no logical contradiction in the theory.' Another common expression of the same basic idea is that since, according to the theory, each point-event is allotted but one single value of the t-co-ordinate, clocks obviously *may* behave so as to give all the readings which the theory requires, and therefore any *a priori* 'disproof' of this *must* be fallacious.

This argument well illustrates the extent to which the role of mathematics in physics has come to be misconceived—a most important point on which I shall enlarge presently. All that Born's 'simple fact' can possibly show is that Minkowski diagrams are the geometrical equivalent of the algebra contained in the Lorentz transformation equations: it has no bearing at all on the question whether the diagrams or the equations represent what occurs in nature. The fallacy in the second form of the argument can at once be seen when we realise that, if valid, it makes possible the assignment of *any values at all* to the times of events in any system, provided that each point-event is given only one time in each system and no observations are available to settle the matter. This is indeed possible *provided that each clock is accompanied by a 'demon' who changes the reading from moment to moment as the hypothesis requires.* But when I conclude from (2) that clocks cannot behave as Einstein's theory requires, I mean that similar, uniformly running clocks, those stationary in each system being synchronised in the prescribed manner, cannot without interference record the theoretical 'times of the system' in which they are stationary. The first thing that Einstein did in constructing his theory was to define the 'time of the system' [7, p. 40], which was not a mere instantaneous reading of a unique clock situated at a single event, but a characteristic of the event determinable by any clock fixed in the system provided that it was synchronised with the standard clock at the origin; otherwise, the word 'synchronisation' would be meaningless. It is true that, in order to make clear how the 'time of the system' was to be determined, he pictured a clock at every point, but this was a mere expository device: to allow it to restrict in any way the universality, throughout the system, of the time of any event would be equivalent to denying any meaning to a, b, c . . . apart from the apple, bell, cat . . . which helped us to understand their application. Einstein himself specifically disowned this childish misconception. Writing, with Infeld, on this point he said [10]:

> When discussing measurements in classical mechanics, we used one clock for all co-ordinate systems. Here we have many clocks in each co-ordinate system. This difference is unimportant. One clock was sufficient, but nobody could object to the use of many, so long as they behave as decent synchronized clocks should.

B. The Role of Mathematics in Physics. The general fallacy of which these arguments are particular examples is one of the greatest dangers in current physical science, and needs to be brought clearly to light. The proper function of mathematics in physical research (I am not speaking of pure mathematics, unrelated to experience) is to enable one to see precisely all that is contained in and implied by any particular idea which one may form about the metrical relations existing in the world; but mathematics can pass no judgment whatever on the rightness or wrongness of that idea except in so far as the implications which it reveals may serve, by comparison with experience, to make such judgment possible. The moment mathematics assumes the primary instead of the secondary role in research, the research becomes unscientific and full of danger—and this notwithstanding the fact that a mathematical formula may often be a useful pointer to possibilities which might not otherwise have been suspected. Often—perhaps usually—the mathematical machinery employed is over-powerful for its purpose, and yields results both valid and invalid in relation to experience.

The most familiar example of this occurs in exercises in elementary algebra, in which one is required to find, say, the number of men required to do a certain piece of work under given conditions. A quadratic equation is formed, which has two roots—say 10, and −6. In such a case we simply reject the second result without hesitation, and accept the first as the solution of our problem. But mathematically both results have equal validity; there is not the slightest mathematical reason for not concluding that we could get the extra job done either by adding 10 men to our gang or subtracting 6 men from it. In this case we do not need to make an experiment to prove that the latter procedure will be ineffective, but that is *only because of the weight of previous experience.* Without that, if we allow mathematics to take the initiative, we must conclude that both results have the same applicability to fact. I have no doubt that, if we had had no general experience to the contrary, many mathematical physicists would today assert dogmatically that Achilles could never overtake the tortoise.

In the case of co-ordinate transformations, we are without sufficient experience to decide between at least two possibilities—the Galilean and the Lorentz transformations. Mathematically they both preserve the relativity principle. We know that only one of them can correspond to the actual behaviour of clocks and measuring rods in relative motion, but we have no experience at sufficiently high velocities to decide experimentally between them. We therefore cannot say, from direct experience, which corresponds to the 10 and which to the −6 men. To assert that either of them must be true because it is mathematically self-consistent is to allow mathematics to dictate to physics, and to expose ourselves to the possibility of errors as absurd as that of expecting more work to be done by employing fewer men.

Naturally, the dangers attending this kind of procedure are greatest in fields where experience is scantiest, and one example from cosmology will show the actuality of its menace in an extreme form. Much work is now being done on the properties of a conceptual universe conforming to the mathematics of general relativity, with the symbol, p, representing pressure taking a negative value. But, in the theory employed, the symbols representing pressure, density, etc., existing in the universe can be identified with those physical quantities only on the condition that 'pressure' has an interpretation that precludes its having a negative value. The assumption made, therefore, is equivalent to the supposition that, because workmen can be represented by algebraic symbols and such symbols can have negative values, therefore 'negative men' exist.

3 *Relation of the Special Relativity Theory to Experiment*

A. The Origin of the Theory. It is generally held that Einstein's special relativity theory has received a large amount of experimental support: in fact, it has received none at all. The situation can be expressed summarily in this way. At the beginning of this century the existing theories of mechanics and electromagnetism were incompatible with one another. Einstein [4], in 1905, proposed an amendment of mechanics, the effects of which, however, would be perceptible only at velocities far beyond practical realisation. If the amendment were justified it would succeed in making the electromagnetic equations, like those of mechanics, relativistic, and so remove the incompatibility; but, clearly, the only possible test of such a theory was a mechanical one. It was framed in order to justify electromagnetic theory, so that to use electromagnetic theory to justify it would be to argue in a circle. All that electromagnetic experiments could show was whether the theory, *if* it was correct, achieved its purpose: in other words, they could show that either Newtonian mechanics was wrong and the classical electromagnetic equations right, or the classical electromagnetic equations were wrong and the Lorentz transformation was (over the range covered by the experiments) the true correcting factor, but they could not possibly decide between these alternatives. But all the experiments so far performed on this matter have been electromagnetic in character, and therefore afford no information concerning the validity of Einstein's theory. For that a purely kinematical experiment, totally independent of electromagnetic theory, is essential.

The reason for the remarkable oversight of this simple fact lies in the history of the subject. Einstein's and Lorentz's theories correspond, respectively, to the two alternatives just mentioned. For Einstein, the Lorentz transformation formulae express the actual physical relations between moving measuring rods and clocks, and the classical electromagnetic equations express the actual relations between electromagnetic quantities and the readings of those instruments; but the physical assumption on which the electromagnetic equations were built (that of a static ether with respect to which the velocity of a body had an absolute, and in principle measurable, value) was false. For Lorentz, on the other hand, Newtonian mechanics, involving the Galilean transformation, was true, but motion through the actually existing ether distorted measuring instruments so that their readings accorded with the Lorentz transformation. The electromagnetic equations therefore represented the relations between quantities which would actually be measured in experiments, provided that the effect of this distortion was taken into account. There were thus two theories, essentially different in the physical sense, but identical in the mathematical machinery employed.

It is not generally realised now that the term 'relativity', which is universally and rightly attached to Einstein's theory, was for many years attached to Lorentz's; and, indeed, Einstein's theory attracted little attention until the success of his *general* relativity theory in 1919 brought it into prominence. The two foremost critics of the common mathematical formulation, for example—Ritz, who opposed it, and Poincaré, who supported it—scarcely acknowledge Einstein's part in the matter at all. Ritz [11], writing in 1908, directs his attack wholly against Lorentz, and Poincaré, in an address entitled 'L'Espace et le Temps', delivered shortly before his death in 1912, explains at length 'le principe de relativité physique' without mentioning the name of Einstein. He writes, for example [12]:

> Quelle est alors la révolution qui est due aux récents progrès de la Physique? Le principe de relativité, sous sa forme ancienne, a dû être abandonné, il est remplacé par le principe de relativité de Lorentz. Ce sont les transformations du ' groupe de Lorentz ' qui n'altèrent pas les équations différentielles de la Dynamique.

It was thus the physical ideas of Lorentz, and not those of Einstein, which were associated with the so-called relativity theory (Lorentz's theory, requiring a static ether with respect to which motion was absolute, was of course not in fact a relativity theory; it merely showed why phenomena occurred *as though* motion were only relative), and when the time came for the authorship of the theory to be transferred to Einstein, Lorentz's physical ideas were automatically transferred with it and mingled with the incompatible ideas of Einstein. Thus a state of complete confusion was generated, in which we are still involved, which exhibits an indeterminate mixture of conflicting physical conceptions associated with a single set of equations. Unfortunately, Einstein himself was not entirely free from the confusion, though his ideas were far clearer than those of his putative followers today. Lorentz appears to have understood the position fully. ' The theory of relativity ', he wrote in the last year of his life [13], ' is really solely Einstein's work ', and he never accepted it [14].

The mathematical similarity and the physical difference of the two theories is most simply manifested by the following experiment. Suppose two relatively stationary clocks, A and B, at a distance apart and, for simplicity, stationary in the ether if there is one, are synchronised by light signals in the manner common to the theories. Let a third clock, C, originally agreeing with A, move to B at high velocity. On both theories C will show the same earlier reading than B on arrival, but the reasons for this are quite different. On Lorentz's theory C will work more slowly because of its motion: on Einstein's theory there is no change in C, but the definition by which B is set puts its reading ahead of C's. (The difference, of course, would be revealed if C returned to A, for on Lorentz's theory it would continue to work slowly and so would exhibit ' asymmetrical aging ', whereas on Einstein's theory there is no reason why A and C should disagree; but since this is still the centre of so much confusion, it is perhaps wiser not to introduce it here.)

That this account of Einstein's theory is a true one was shown plainly by Einstein himself, in answering the objection that he had given an unwarranted uniqueness to light, which was but one among many physical phenomena. (I well remember this objection being advanced by A. N. Whitehead, whose lectures I was attending.) Lecturing at Princeton in 1921 he said this [15]:

> The theory of relativity is often criticized for giving, without justification, a central theoretical rôle to the propagation of light, in that it founds the concept of time upon the law of propagation of light. The situation, however, is somewhat as follows. In order to give physical significance to the concept of time, processes of some kind are required which enable relations to be established between different places. It is immaterial what kind of processes one chooses for such a definition of time. It is advantageous, however, for the theory, to choose only those processes concerning which we know something certain. This holds for the propagation of light *in vacuo* in a higher degree than for any other process which could be considered, thanks to the investigations of Maxwell and H. A. Lorentz.

This makes the matter perfectly clear. We can set B as we like, but we are most likely to get the setting which would be verified in the actual experiment if we choose that which accords with our idea of what is most certain in physics, namely, the Maxwell-Lorentz electromagnetic equations. The mechanical part of the theory, in short, is constructed so as to conform to classical electromagnetic theory.[1]

B. The Relevance of Experiment to the Theory. What, then, in the light of these considerations, must be a *sine qua non* of any experiment designed to decide between classical (Maxwell-Lorentz) electromagnetism and classical (Newtonian) mechanics? Clearly there must be no presupposition that either is necessarily right, and so the experiment must be *described* in terms not involving such presupposition: only in that case can its *result* have any bearing on the point to be decided. Yet, incredible as it may seem, the best known of such experiments—that of Michelson and Morley—as it is universally understood, fails to meet this condition. The original issue was clear: on electromagnetic theory there should be a fringe shift, caused by the change of motion of the Earth through the ether; on Newtonian mechanical theory (first law of motion) there should be none, since all states of uniform motion are experimentally indistinguishable. The experiment showed no fringe shift: hence electromagnetic theory was disproved.

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[1] It is a fact of historical—and more than historical—interest that the earliest criticisms of Einstein's special theory were directed against the assertion that the time of one event in relation to another was a matter of free definition. It was held to be something absolute, which could not be tampered with, and by the time that this misconception was effectively removed, the critics—mostly philosophers—apparently had no energy left to consider the second, quite independent, question, whether the particular definition which Einstein chose was an effective one. It was welcome to physicists since it saved electromagnetic theory and was apparently beyond experimental refutation, so it came automatically to be regarded as a successful candidate for acceptance, though actually only its eligibility to compete had been established. This misunderstanding still persists. Many critics of my argument against the theory seem to think that in denying that Einstein's definition can pass the test of experience, I am denying the legitimacy of its candidature. I know very well that it must not be taken for granted, without examination, that simultaneity is absolute: what I say is that examination shows that it must be so.

But in fact this is not how the experiment is described. In all the accounts I know, including that of Einstein [16], it is represented as a comparison of the times of travel of something (often, metaphorically, a swimmer) moving up-and-down and across stream, and then calculations are made of the effect of motion on those times. But no times are measured at all. No clock comes into the experiment, so whatever effect motion may have on clocks can play no legitimate part in interpreting the result.

How, then, does this extraneous consideration of time get into the description? Only by the assumption that light is in fact an electromagnetic wave obeying the laws of classical electromagnetic theory. It has a velocity, c, a frequency, n, and a wavelength, λ, and these are related by the equation, $c = n\lambda$. The 'interference' fringes, which are the things actually observed, are interpreted in terms of this conception, and so translated into functions of time because time is involved in the ideas of frequency and velocity. Before conclusions are drawn from the result of the experiment, therefore, it has already been transformed into something which has no meaning unless the requirements of electromagnetic theory are accepted, and then naturally the only explanations of the result which are available are those which require classical mechanics to be wrong because it does not accord with electromagnetic theory (Einstein), or mechanical measurements to be wrong because they do not accord with electromagnetic theory (Lorentz). The most obvious explanation—that the theory which implies the equation, $c = n\lambda$, is wrong—is made impossible because the experiment itself is described in terms which kill it before birth.

Yet it is not difficult to see that even if classical electromagnetic theory is true—if only to the degree that it demands the equation, $c = n\lambda$—then Einstein's mechanics cannot succeed in reconciling it with observation. For this it is necessary to accept merely that there is something objective called *light* which has an objective characteristic represented by λ (we need not even suppose that it is the length of a wave; it may have a quantum or any other specification, though for convenience we may call it 'wavelength'), and then the failure of Einstein's mechanics follows inescapably.

Suppose two widely separated, relatively stationary sources, S_1 and S_2, of the same kind of monochromatic light, and half-way between them two observers, A and B, side by side, initially at rest with respect to S_1 and S_2. They both observe a spectrum line from each source, and these lines occupy the same position in the spectrum. They can in principle measure c and λ and deduce n so as to satisfy the equation, $c = n\lambda$. Now let B move rapidly towards S_1. Then our experience of the Doppler effect compels us to conclude that he will see the light from S_1 displaced in one direction and that from S_2 displaced in the opposite direction. What is the cause of these displacements? Nothing has happened to the wavelength of the light because the light has not been disturbed—and, indeed, A, who has not moved, can verify that it is unchanged. Furthermore, according to the Lorentz transformation, the velocity of the light with respect to B remains c, as before. Hence, if the equation, $c = n\lambda$, is still true, n also must remain constant.

What, then, causes the spectrum shift which B observes? It is not anything that happens to the light because nothing happens to it. It is not anything that happens to B's instruments on account of their motion because that would make the light from S_1 and S_2 appear to suffer the same change, and they appear to change differently. It is nothing that happens to the relation between the light and B because, on Einstein's theory, in relation to light all slower motions are indistinguishable. On Newtonian mechanics (Galilean transformation) the velocity of the light with respect to B, and therefore the frequency, n, would vary with the motion of B—a phenomenon quite familiar with sound waves—so the equation, $c = n\lambda$, would be applicable to B's observations both before and after he begins to move. On Einstein's theory it clearly cannot be, and yet Einstein's theory, as he stated, was built on the supposition that the theory which requires that equation is certainly true.

For the reasons given in the previous section, I do not consider that an experimental test of Einstein's theory is necessary, but since it would obviously be far more convincing to scientists than a piece of pure reasoning, it is very desirable that it should be made, and therefore that the conditions which it must satisfy are clearly understood. Of these, the foregoing considerations show that the primary, and quite inescapable, condition is that the experiment must not in any way, directly or indirectly, involve an appeal to electromagnetic theory; otherwise any argument from the experiment leading to the conclusion that the theory is correct must inevitably be circular. Since it has not yet proved possible to experiment in the laboratory with mechanical velocities high enough to distinguish between the requirements of the Lorentz and the Galilean transformations, a mechanical test of the theory has yet to be performed (astronomical tests are possible but have not yet been made: such tests of Einstein's second postulate have been attempted and will be referred to presently). But there is one important qualification to be made to the apparent implications of this fact. Although no experiment involving electromagnetic theory can support the special theory of relativity, such an experiment might disprove it. The theory, as we have seen, is an *ad hoc* construction to enable electromagnetic theory to be maintained notwithstanding experiments (such as that of Michelson and Morley) which appear to disprove it; hence a purely mechanical experiment is needed to confirm it. But an electromagnetic experiment which, even with the Lorentz transformation taken into account, still violated theoretical expectation, would be fatal. This is important in connection with the recent experiment of Kantor [17] to which reference is made below.

C. Experimental Tests of Einstein's Second Postulate. A number of experiments have recently been made to test, not Einstein's theory *in toto*, but its second postulate, namely, that light issues from its source with the same velocity, in any inertial co-ordinate system, whatever the velocity of the source in that system. This has long been considered proved from observations of binary stars [18], but it is now clear that this result is not conclusive [19, 20]. Accordingly, further experiments have been made, among which may be cited those of Bonch-Bruevich [21], Kantor [17], Sadeh [22], Alväger, Nilsson and Kjellman [23], and some (so far as I know) unpublished experiments whose results have been communicated to me privately and which have recently been referred to by Sciama [24], in which the velocities of photons issuing from stationary and fast-moving π-mesons have been compared. In view of the inferences (some of which contradict one another) which have been drawn from these experiments, it is important to be clear on two points: first, the relevance of the experiments to Einstein's second postulate; and second, the effect of the proof, or disproof, of the postulate on the complete theory.

With regard to the first point, all that has been said about the necessary freedom from electromagnetic assumptions applies to tests of the second postulate as well as to those of the whole theory. The postulate is adopted as part of the basis of a *kinematic* theory, so that ' velocity ' must be understood in a kinematic sense, and this requires that the source of light must be an identifiable body, having a definite position in space at each successive instant, the whole sequence of positions being consistent with the velocity assigned to the body. This consideration at once rules out the experiments of Sadeh, Alväger, Nilsson and Kjellman, and those cited by Sciama, as irrelevant (though, of course, far from valueless as sources of general information). In these experiments the sources were fundamental particles, and not only have such ' particles ' no individual identity, since they do not obey classical statistics—a conclusive disqualification in itself—but also, when one reflects that, when operating in vacuum tubes, they must be assumed to be accelerated from rest up to a velocity near c within a distance negligible compared with the length of the tube (since their Doppler effect indicates a uniform velocity throughout the tube), and that the same particles must then creep back and repeat the process over and over again, or that an unlimited supply must queue up to make the journey at a uniform rate and then pass out of observation—it becomes hardly possible to regard the representation of them as bullets on a small scale as anything but metaphorical. The properties which they are supposed to possess, and even their very ' existence ', depend on assumptions which, no matter how plausible, include a literal acceptance of the picture associated with electromagnetic theory, so they are at once disqualified as ' sources ' in the sense implied in the statement of the second postulate.

The experiment of Kantor is likewise disqualified since, being an interference experiment, it implies the acceptance and orthodox interpretation of the equation, $c = n\lambda$, which has already been discussed. Moreover, there is considerable doubt, which is reinforced by an observation of James and Sternberg [25], about his assumption that light, passing through a transparent medium, travels thereafter as though it had been absorbed and re-emitted from the particles of the medium. However, the experiment, as is shown below, is significant with regard to the special relativity theory as a whole, though its bearing on the second postulate is questionable.

The only test of the second postulate which is valid in principle is that of Bonch-Bruevich, who compared the velocities of beams of light from opposite limbs of the rotating Sun. These are legitimate sources, but unfortunately Bonch-Bruevich reflected the beams into a common direction before comparing them, and in these circumstances they would be expected to show the same velocity, even though the velocities differed before reflection, according to the theory of Ritz which denies the second postulate [26]. Moreover, the method used for determining the velocities was doubtfully free from conceptions of the nature of light depending upon electromagnetic theory for their acceptability, and was therefore, though not necessarily invalid, at least less satisfying than a direct comparison of times of flight as suggested elsewhere [27].

The net result, therefore, is that Einstein's second postulate is still untested experimentally, and therefore, notwithstanding their apparently conflicting results, all the experiments mentioned above might yield authentic information since the apparent contradictions might arise from the false assumption that they test the same thing. It is now necessary to point out, however, that a confirmation of the postulate would be anything but conclusive regarding the special theory as a whole. That theory follows logically from the two postulates—that of relativity and that of constant light velocity—and the chosen definition of distant times, i.e. of clock synchronisation in a single co-ordinate system. If the second postulate is verified, then, to that extent, both Einstein's theory and that of Lorentz would be supported, for they differ only in regard to the first postulate. But even if Lorentz's theory were then disproved, and both of Einstein's postulates established, his theory might still fail on account of its definition of distant times. This may be seen most easily by considering the parallel case of sound waves, which are known to behave in the manner described in the second postulate. Imagine two airmen, travelling at the same high velocity, one ahead of the other, who synchronise their clocks by means of sound signals. They would deduce the equivalent of the Lorentz transformation, with the velocity of sound in place of that of light, for the relation beween their observations and those of observers on the ground, and we can see at once the difficulties that would arise: they would deduce, for example, that velocities greater than that of sound would be impossible and that, if they travelled with the speed of sound, all earthbound clocks would cease to work. In the same way, even if Einstein's two postulates were confirmed, his theory would still be vulnerable on account of its prescription for synchronising clocks.

D. Kantor's Experiment. Assuming, then, that all the experiments referred to above are entirely free from technical errors (a necessary assumption in the present state of knowledge, though the importance of reserving final judgment until they are independently repeated needs no stressing), we must conclude that (Kantor's excepted) they yield no definite conclusion relating to any aspect of the special relativity theory. We must exclude Kantor's experiment from this generalisation because, with the proviso already mentioned concerning its unimpeachability on experimental grounds, it does constitute an experimental proof—the first to be obtained—of the invalidity of the special relativity theory.

In this experiment a momentary beam of light was split into two parts which traversed a circuit in opposite directions and then re-united to form interference fringes. In the path of the beams were placed two thin transparent windows which could be made to move rapidly in the same direction as the light in one beam and therefore in the opposite direction to the light in the other. The positions of the fringes when the windows were stationary were compared with those seen when the windows moved, and a displacement of the order of at least 10^{-1} fringe was observed.

Now on Einstein's theory the shift should have been at most 10^{-4} fringe. The principle involved is that of the well-known Fizeau experiment which is generally adduced in support of the special relativity theory, and from the data given by Kantor, this result follows without question. We are not dependent, therefore, on a decision concerning the relation of the experiment to the second postulate: we can conclude, independently of that, that special relativity theory is untenable because it does not fulfil its purpose of accrediting classical electromagnetic theory, which was its sole claim to acceptance.

4 *Conclusion*

We reach the conclusion, then, that the special relativity theory can no longer be maintained—first, because it requires measuring instruments to behave in impossible ways, and second (if Kantor's result is confirmed), because it violates experiment. Since it was devised to avoid an apparent breakdown of electromagnetic theory, we are left with the alternatives that that theory is in fact untenable or that some supplementary theory is possible that can redeem it. The only such theory yet proposed for this purpose is that of Lorentz, which is highly unlikely since the postulated changes accompanying motion through the ether, which must affect charged and uncharged bodies alike, are entirely *ad hoc*, with no semblance of independent justification, and also, if this theory were correct, it would leave us with the possibility of velocities greater than that of light with which it would be powerless to deal; Lorentz, in fact, entitled his paper [5]: 'Electromagnetic phenomena in a system moving with any velocity less than that of light.' Furthermore, the original dilemma would still remain, that the *laws* of mechanics would be intrinsically relativistic while those of electromagnetism would be non-relativistic, and only electromagnetic *phenomena* would present the appearance of relativity because of the inexplicable device by which nature disguised the fact of motion. Finally, Kantor's observation is beyond explanation, even on Lorentz's theory.

The alternative, that the laws of electromagnetism need reformulation, thus appears almost inescapable, and indeed, quantum phenomena have long been telling us this—though, in view of the apparent justification of the Maxwell-Lorentz theory by special relativity, attempts have naturally been concentrated (without success) on the attempt to reconcile it with such phenomena instead of on the formulation of fundamentally new laws. We must now, however, face the fact that new laws are necessary. It is possible that these may involve only a slight amendment of the Maxwell-Lorentz conceptions, or something radically different. The great success of the Lorentz transformation, regarded as a correcting factor to the classical equations, suggests that, if it could be worked into the structure of the equations instead of being imposed from without by an attempted violation of mechanics which has proved impossible, a satisfactory result might be reached. In this connection, it must be borne in mind on what a narrow experimental basis the Maxwell-Lorentz theory rests. If, for instance, the connection between static and current electricity were such that a current, i, instead of being equivalent to the product, qv, of electricity of charge-density q moving with velocity v, were equivalent to $qv/(1-v^2/c^2)^{\frac{1}{2}}$, the fundamental experiments would appear to be equally well covered and the resulting general equations might achieve all the success hitherto standing to the credit of the Maxwell-Lorentz theory plus special relativity.

On the other hand, Kantor's experiment (it is most regrettable that Kantor did not determine the relation between fringe shift and velocity of windows), as well as certain other considerations, suggest that this, even if successful, could not wholly solve the problem, and the necessity for an entirely new set of conceptions must in all probability be faced. In that case, the whole picture of the electromagnetic field, and of light as a wave travelling through it, must be regarded as a remarkably successful metaphor, but one which is dangerously misleading if regarded as a literal truth. In particular, the conception of light as something (no matter what) travelling progressively from point to adjacent point of space, may be fundamentally fallacious (it is tacitly violated in special relativity theory, in which a beam of light has the same velocity with respect to two bodies themselves in rapid relative motion). What we call the 'velocity' of light is, in fact, some characteristic of a whole 'wave-train': no observation of the continuous progress of an isolated 'pulse' or 'photon' through space has ever been made. The abandonment of the representation of light as a wave-crest or photon moving like a bullet might seem a rash proposal, but the history of physics abounds in examples of apparently unassailable explanations of phenomena which have later given place to explanations wholly different in character. The representation of gravitation as an attraction, the Lorentz explanation of the Zeeman effect, Sommerfeld's derivation of the fine-structure of spectrum lines, as well as the implications of the wave theory of light which are directly contradicted by quantum phenomena, serve to remind us that no theoretical conceptions, however abundantly confirmed, are immune, not merely from changes of detail, but from complete transmutation.

But it is not my purpose here to attempt a re-construction of electromagnetic theory. There are many far better qualified than I for such a task, if only they can be brought to realise its undoubted necessity. Unfortunately, the dogma that a refusal to accept special relativity theory is necessarily based on a misunderstanding of it has become so powerful that it is almost impossible for serious criticism to be read with sufficient care to evoke the awareness that it merits attention.* Mathematics, which should exemplify reason, is instead used to displace it, and a constant critical examination of the relation of mathematical symbols to observations is becoming an out-of-date superfluity. Yet, especially at a time when hazardous experiments have to be undertaken on the basis of theoretical expectations, it is the compelling duty of physicists to put such examination in the forefront of their activity. All I can hope to do is, in some measure, to weaken the force of the dogma and restore the attitude, on which all success in science ultimately depends, of complete submission to the demands of reason and renunciation of all attempts to ignore or misrepresent considerations whose consequences would be unpalatable.

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* H. Arzeliès [28] has expressed this fact with grim naiveté. He divides physicists into two groups—'groupe relativiste (groupe I)', to which he belongs, and 'groupe anti-relativiste' (groupe II), and he proceeds: '*Quelle conclusion adopter?* Mais tout simplement que les deux groupes possèdent vraisemblablement des structures mentales quelque peu différentes; ce qui est évident, tres clair, pour les uns, est obscur *et* absurde pour les autres. *La discussion sort du domaine de la physique ; elle relève de la psychologie expérimentale ou de la psychiatrie.* Je dis cela très sérieusement, sans aucune ironie. . . . De toute façon, continuer à discuter entre physiciens, avec des arguments de physique ou de mathématique, est une perte de temps.' I think this is a just, if somewhat embarrassing, description of the attitude of most of my correspondents on this matter, but this paper is written in the hope that group I contains a remnant who are not satisfied to regard an appeal to reason on this, or any, question of physical theory as a waste of time, and are prepared to accept its verdict, no matter how unexpected or unwelcome.

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REVIEWS

EXPLANATION IN SOCIAL SCIENCE

1 *Introduction*

Robert Brown has written a most important and interesting book.[1] It is a survey and discussion of the different methods of explanation used in the social sciences. The author's thesis seems to be that explanations in the social sciences are comparable to those in the natural sciences in most ways—the social sciences even have some extras, like intentions—and fall behind only in degree of development. Moreover this is argued by a philosopher who seems to be thoroughly familiar with a lot of the literature in the social sciences, and who illustrates each point he makes with ample examples. I want in this review first to summarise the main outline of the book, then to present those criticisms of it I can think of, and finally to set down one or two ideas stimulated by the book.

In the first of the book's two parts, Dr Brown outlines some sorts of questions social scientists ask, and brushes off those who claim that social science describes but does not explain and that the description is moreover historical rather than scientific. He does this by producing examples which show that social science at least purports to explain, and to describe in a non-historical way. Closer investigation of this *prima facie* rebuttal is rightly left to the more detailed part two. In part two the author plunges into an analysis of seven out of the nine kinds of explanation he is able to isolate in the social sciences. The seven are: genetic, intentional, dispositional, explanations in terms of reasons, functional, explanations using empirical generalisations, and explanations using theories. The two left out of this are 'explain yourself' and 'explain to me why we can derive $\bar{\bar{s}}$ from (1) $p \vee q . \supset r$, (2) $\bar{q} \supset \bar{s}$, (3) $\bar{r}$'. The 'explanation' demanded by the phrase 'explain yourself' is more of an elaboration or clarification; the explanation of the derivation would be part of a course in logic and not really special, or especially interesting, to social science.

Brown devotes a separate chapter to each of the seven 'methods of explanation' listed and it will be as well to go over them one by one. (In order to simplify some rather involved discussion I will try to use only two examples of explanations of social events in this expository section. They are not Brown's examples. The first is a lynching. Let us take the case of any small town where, one night, a mob raided the gaol and dragged out and hanged a man awaiting trial. Let us take as a second case that of a first fruits ceremony, a ritual of dance and prayer which must be performed in some societies before anyone is allowed to cook or eat the current harvest.)

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[1] *Explanation in Social Science.* By Robert Brown. Routledge and Kegan Paul, London, 1963. Pp. vii+198. 25s.

2 *Exposition*

Genetic. 'Most work in the social sciences, like most work in history and in the natural sciences, is not directly concerned with the furnishing of explanations' (p. 47). Identification, classification, description, measuring, and reporting are tasks which social scientists perform in addition to explaining. One form of explanation they use is genetic explanation. Examples would be: 'What is the origin of this habit of lynching?' 'It began at a time when there was no law and order in this territory and the citizens got used to having to deal with lawlessness themselves.' A genetic explanation is here given of the 'habit'. 'What is the origin of the first fruits ceremony?' 'It perhaps stems from an ancient belief that harvest produce is a gift of god which must be acknowledged before consumption.' Genetic explanations can include reasons (the felt lack of law and order), dispositions (their habit of dealing with lawlessness themselves) and intentions (to see justice is done), but not laws and functions. Genetic explanations are in fact to be contrasted with law-explanations. Genetic explanations 'are statements reporting or describing specific events, processes and situations which are either dated or datable' (p. 56); they are statements of initial conditions.

Intentions. For example, 'What was his intention in joining the lynch party?' 'To see justice done.' Remarking the paucity of explicit intention-explanations in the works of social scientists, Brown argues that to attribute intentions we must ascribe to the actor: (*a*) knowledge of his goal (that justice be done; that gods be propitiated); (*b*) knowledge of the connection between his goal and the means employed (mob action will effect justice; that prayers and dances propitiate gods); (*c*) belief that he himself would attempt to achieve this goal when possible (p. 67). Moreover, only individuals have intentions and the ascription of intentions to groups is misleading.

> In summary, then, these properties of intention statements are: (1) the statements which they entail by themselves are limited to belief statements about goals and to statements of belief and knowledge about the means to these goals, (2) intention statements cannot be logically derived from non-intention statements alone, (3) one intention statement by itself can entail another only when they both mention or refer to the same goal (p. 70).

Dispositions. For example, 'Why were lynch parties so frequent in the Southern part of the United States? Because the population there is disposed to dispense justice in that way.' Dispositions are not the same as intentions since goal-orientated dispositions are not the paradigm of dispositions. One can be disposed to jump at loud noises as well as disposed to join lynch parties. Intention explanations which presuppose a goal cannot be said always to presuppose a disposition to seek that goal. For example, 'He crossed the street because he intended to get tobacco,' may be a true explanation although he does not have the general disposition to get tobacco (he may be getting it for someone else), and although even if he did, crossing the street is not an appropriate way to get it (because there are no tobacconists over there); it is enough that he *believes* it is. Just as intentions do not entail dispositions, many exercises of dispositions are not intentional. From some disposition explanations we can derive intention explanations, but not from unlearned reflexes, habits, and motives. Dispositions are not the same as capabilities either: I may be disposed to sing opera in the bath even though I am incapable of singing a correct note; I may be disposed to seek justice through lynching parties even though I am incapable of acting justly when surrounded by lynch-party emotions; I may be capable of being a good pianist but, having no disposition to practice, I never shall be. Talk of dispositions to conform to social rules is platitudinous as this disposition to conform is equivalent to society and its conventions, no less.

Reasons. For example, 'Did he have any reasons for joining the lynch party?' 'He believed justice would not be done otherwise.' Reasons are explanations of actions not of 'differences between classes of actions, or . . . increases in the members of a class of action, or. . . rates of increase and comparative rates' (p. 100).

> (1) they apply only to actions intended by the agents, (2) they must be assignable to individual agents or to individual agents as members of groups, (3) they are logically independent of functional—in the sense of 'causal effect'—explanations, (4) they require evidence concerning (*a*) the agent's goal (*b*) his ability to formulate the relevant arguments and (*c*) the decisive effect of that argument upon his action, (5) they can be used only to a limited extent in the explaining of other reason explanations (p. 104).

The social scientist may choose to explain why he thinks the reasons cited are not in fact the agent's reasons and he may explain these reasons by generalisations (e.g. people tend to rationalise their race prejudices).

Functions. For example, 'What is the function of lynch parties?' 'They create social solidarity.' 'What is the function of first fruits ceremonies?' 'They " enable men with expert knowledge to control these activities, so that labour will be efficiently co-ordinated, the premature dissipation of food stocks prevented, and perhaps also the danger to health obviated which might be caused by eating unripe crops ".'[1] Functional explanations can be of two kinds: purposes (what is the function of radar?—to track things); or system-maintenance (what is the function of the first-fruits ceremony?), i.e. their causal role in a negative feedback system. Brown gives a sharp and sustained critique of some social anthropology examples from Fortes, Radcliffe-Brown, and Homans and Schneider, and argues that unless there is a feedback we can say ' one effect of x is y ' is no explanation at all.

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[1] Quoted from S. F. Nadel, *The Foundations of Social Anthropology*, London, 1951, p. 274. This particular functional explanation is more Malinowskian (the needs of societies) than Durkheimian (social solidarity).

> The three questions that must be answered if we are to explain in terms of functions remain unanswered and, often, unanswerable. They are: 'What property is being maintained in a steady state?' 'Which internal properties are the variables, how can they be measured, and what are their values and ranges?' 'Which external conditions are assumed to be constant and which to vary within specifiable limits?' (p. 127).

Function explanations are rarely used because they have to fulfil such stringent conditions.

Empirical generalisations. For example, 'Why did the lynch-party beat-up that bystander?' ' All lynch mobs get out of control.' Whether the social sciences have well-tested empirical laws is the social scientists' business—they are the best judges of the severity of the tests. Whether they have laws which are logically impeccable seems obviously to require an affirmative answer. Yet examples on examination turn out to be somewhat untestably ambiguous or elementary, and empirical rather than theoretical. The untestability of the generalisations is sometimes due to their being implicit and sometimes to analytic statements masquerading as empirical ones. Against [illegible] critics Brown argues that there are laws in social sciences which are uni[illegible]l, are testable (at least in principle) and are invariant with respect to [illegible] but that laws in any science should never be so carefully scrutinised [illegible]ndently of their accompanying glosses or ' text '. He criticises those [illegible]nk generalisations only predict but do not explain.

[illegible]ories. There are various uses of the word ' theory '. In one such it is synonymous with ' explanation '. In another it means the scientists' frame of reference, which determines his problem-interests and methods of dealing with them. ' Theory ' is also sometimes used of classification schemes, but these, too, are usefully constructed only with reference to problems.

The two usages Brown concentrates on are first ' theory ' referring to a statement which explains by means of abstract, unobservable, or ' theoretical ' entities. These are important because some people argue that only such entities cut off chains of ' why ' questions, only they allow us to end discussion by saying ' that is the way the world is constructed '. Social science uses abstract idealisations (perfect market, ideal types) and unobservables (social relations, Id) and extreme cases (folk society, urban society). But talk of such explanatory concepts being ultimate should be avoided; it is vague and pretentious. The second usage of ' theory ' Brown tackles is that of developed deductive systems. He first distinguishes between uninterpreted calculi applied to reality and informal theories being formalised. Social science has both, e.g. economics (uninterpreted calculi being applied) and informal theories which may be formalised. It is not highly developed in this direction yet, that is all.

3 *Critical Comments*

I first read this book when it was a London Ph.D. thesis, dated, as I recall, 1954. Dr Brown does not mention this origin of his work, doubtless because it has been completely revised and rewritten since then: yet I still recognised it as having structural similarities with that earlier work and sections of its discussion seemed to have been left almost intact. The main changes I can detect are a jettisoning from the thesis of linguistic philosophy techniques, a considerable increase in the number of illustrations, and a general reorientation of the book so that it constitutes a *riposte* to critics of explanation or the lack of it in the social sciences. All these changes are improvements, except perhaps the huge scale of the increase in examples (the effect of which I mention below).

The value of the book, as I see it, is that the author writes as a trained philosopher and is able therefore to sort out many logical muddles which surround explanation in social science, steering carefully clear of one area—the logical features of scientific laws and theories—to which a number of books have already been devoted. His work should be an eye-opener fo some prejudiced critics of the social sciences, since he is able to rebut most their charges. But it should also be a shock to those social scientists somewhat uncritically rely on, e.g. functional explanations, or who yea their disciplines to be more like physics but whose yearning is ba admiration of certain characteristics of physics which others have ar not important or are to be imitated only at peril. Brown's book ac quite the best general survey of the field, although I do not find it al satisfactory.

In particular, I am unhappy about the fact that the book assumes a familiarity with the literature on the logic of explanation. Much of it cussion turns on the reader understanding the so-called deductive-no logical view of the nature of explanation. The trouble is, only philosoph will be familiar with this area, yet the book is not for them. As part of the International Library of Sociology and Social Reconstruction it is clearly meant primarily for social scientists. But they are precisely those who need to be familiarised with the mundane logical points Brown does not explain. A brief summary (not a lengthy discussion) of the main points of the literature would have enhanced the book a good deal and made it more suitable for its putative audience.

As to the argument of the book, I will state two very minor criticisms first.

There are two things that worry me in the chapter on Intentions. The first is an argument taken from Miss Anscombe, the second a point about intention-statements not being derivable from statements not containing intentions. Miss Anscombe is quoted as arguing that we cannot unintentionally pay our respects, take aim at a target, cultivate a plant, cook a meal, or build a garage. We are told that ordinary language here is intentional: 'Once we give an account of an action by means of such a phrase there is no longer any room for an unintentional explanation' (p. 61). I do not see this. Consider these sentences: 'I only intended to knock up a snack but I got so many ideas that without realising it *I ended up cooking a meal.*' 'He rushed into the room and before I knew what was happening I had aimed the gun at him and pulled the trigger. I had no intention of aiming at him.' 'Not knowing the customs of the society he remonstrated with his hosts using gestures which to them signified that he wished to depart and was *paying his respects.*' These seem to me to be uses of the phrases in connection with actions which are explicitly unintentional. Perhaps I am wrong but these counter-examples are so obvious Dr Brown should have treated us to a discussion of them. It is not so easy to fit 'building a garage' into an unintentional context, so perhaps the general point does hold anyway.[1]

The second criticism is a still more minor one. Brown says that unless an intention-statement is in the explicans then it cannot be validly derived in the explicandum. Strictly speaking this is true, but in informal cases not too important. Consider the schema:

(1) All potlatches are prestigious
(2) *X* gave a potlatch
(3) *X* knew (1) when he gave the potlatch
(4) Therefore *X* knew he would gain prestige when he gave the potlatch.

We still have no '*guarantee*' (Brown's word) that when *X* gave the potlatch he *intended* to gain prestige. Might we not, however, reasonably guess that he did *until presented with criticism?*

Despite all that is interesting in the book, I have said I was left with a certain feeling of dissatisfaction. This was partly provoked, I think, by the organisation. In part two Dr Brown considers seven 'methods of explanation'. Origins, Intentions, Dispositions, Reasons, Functions, Generalisations, and Theories are lumped together as 'methods'. By calling them methods, Brown seems to imply that special rules govern the use and the testing of each of these kinds of explanation; moreover, rules also govern their interrelations. For example, what can be explained by intentions and what by empirical generalisations are different even if they overlap. Moreover testing '*X* intended to gain prestige' would require a different sort of procedure than would testing, say, ' All potlatches are prestigious '. Yet, intuitively, these ' methods ' sit oddly together. Generalisations and theories connote kinds of statements that are often used in explanations. The others are not kinds of statement but kinds of theories or generalisations; i.e. generalisations or theories utilising origins, intentions, dispositions, reasons, and functions. Generalisations and theories are *forms* of statements, the rest refer to the *content* of statements. Again, intentions, dispositions, and reasons are all usually used in connection with the explanation of individual action and decision. While functions are more often used to explain institutions. These differences puzzle me; I do not see why Brown treats all these ' methods of explanation ' on a par.

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[1] I cannot but remark that I find the appeal to what we ordinarily say or mean, and to our inability to conceive of something (e.g. building a garage unintentionally), an extremely weak type of argument.

Brown does subdivide these seven when he suggests the opposition between genetic and law (or theory) explanations. But here we come back to my point about his not adequately sketching in the literature on explanation. What I think he means here is this. Genetic or historical explanation emphasises initial conditions, while law or theoretical explanation emphasises universal laws. Here I must refer to the deductive model of explanation, familiar to readers of this *Journal*, which looks like this:

$$\begin{array}{ll} \left.\begin{array}{ll}\text{Universal law(s)} & H\\[1ex] \text{Facts or initial conditions} & I\end{array}\right\} & (\textit{explicans}) \\ \hline \text{Statement to be explained}\quad O\} & (\textit{explicandum})\end{array}$$

In words: from a statement of a law (or laws) plus some statements of fact (or initial conditions), the statement to be explained can be validly deduced. From ' all planets move in ellipses ', plus ' Earth is a planet ' we can derive ' Earth moves in an ellipse '. From ' all men die when their heads are cut off ' plus ' King Charles I of England had his head cut off ' and ' King Charles I of England was a man ' we can derive ' King Charles I of England died '.[1]

Scientists, we are inclined to say, search for laws like ' all planets move in ellipses '; historians *use* laws like ' all men die when their heads are cut off ', but their real interest is in initial conditions: in how and why it came about that Charles I of England had his head cut off. Here then lies the difference of emphasis between genetic and law explanations, and the basis on which, as I understand him, Brown discriminates them. Simply to discriminate genetic from law explanations does not strike me as enough, however, because individual and social *explicanda* are not thus discriminated; and, on top of that, Brown utilises implicitly a further method of explanation—the logic of the situation, which he nowhere explicitly discusses and which seems to fit into neither category.

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[1] This deductive analysis of explanation was first published by K. R. Popper in *Logik der Forschung*, Vienna, 1934, see section 12 of the English translation, *The Logic of Scientific Discovery*, London, 1959. C. G. Hempel and P. Oppenheim claim that it is ' by no means novel ' in footnote 4 to their paper expounding a similar view ' The Logic of Explanation ' in *Readings in the Philosophy of Science*, ed. H. Feigl and M. Brodbeck, New York 1953. But see W. W. Bartley III, this *Journal*, 13, 1 ff.

Simply to differentiate genetic from law explanations is not enough. Certainly these two emphasise different parts of the explicans. But Brown's set of ' methods of explanation ', while it may be exhaustively divisible into genetic-type and law-type explanations, is also capable of other divisions by means of different criteria, which expose other fundamental problems in explanation in social science.

What I am particularly concerned about is Brown's complete failure to discuss the holism-individualism issue. Apart from saying that intentions sit uneasily on groups and displaying an implicitly individualistic attitude throughout, Brown does not deal with this issue at all. The reason why I think he should is because there are important metaphysical and, as corollaries, methodological, issues involved. These metaphysical issues seem to me as important as the logical problems of explanation in social science. Brown nowhere refers to any work which deals with these issues to his satisfaction; thus I assume it simply to be an oversight.

Yet these metaphysical issues are important; they affect what in social science is explained and how it is explained. Holism is that view of society which claims that there are autonomous social wholes with aims; individualism is the position that only human individuals have aims. These two positions clash in a whole series of ways. Holists tend to be interested in the broader picture, historical patterns, wars, revolutions, hydraulic civilisations, and the like. Individualists may treat of these things, but they also look at the minutiae of day-to-day social behaviour, and always their explanations are in principle reducible to those involving typical individuals acting to realise certain ends in a situation composed of other individuals and of institutions and traditions, or to macro-effects of many individuals so acting, or to the unintended effects of individuals so acting. Holists tend to look for social wholes and to use them as explicans; individualists tend to put social wholes in the category of explicanda. What is explanation for one is problem for the other.

Take as an example the first fruits ceremony. Do we explain it holistically as something the social structure demands? Do we explain it genetically with reference to the history and traditions of the society? Do we explain it individualistically as the concerted actions of a number of individuals who happen to share common religious beliefs about first fruits and the ceremonies associated with them? Again, is a lynching a traditional method of meting out justice (genetic); a typical excrescence on the capitalist economic structure (holistic); or the actions of a group of men who believe that justice will not be done unless they see to it (individualistic)? In each of these cases we can see how the genetic explanation and the individualistic explanation complement each other, whereas the holistic explanation stands aside as something special and different.

The issue does come up briefly at one point in Brown's book where functionalist holism is concerned. This would be illustrated by my first example where the first fruits ceremony is explained because the social sructure demands it. Brown is quite clear that such a purported explanation is no explanation at all unless the system is one that can be shown to contain negative feedback. Otherwise we are involved in postulating social goals in societies which are in no sense being 'directed'.

On the general issue of holism, however, Brown is reticent. It is an important metaphysical issue to decide what we are prepared to explain, and in what terms we are prepared to explain it. Social events can be explained in terms of other social events, individual decisions can be explained in terms of social events, and so on. But the key metaphysical question is whether social action can be explained other-than-individualistically.[1]

While Brown is clearly an individualist, he never raises the issue explicitly. He does not discuss, for example, whether inexorable historical laws, or social forces, can be used as explanations. They can be used as explanations in the logical sense that they explain anything which can be validly derived from them. What I mean is whether we can allow such laws within *satisfactory* explanation.[2]

There is a possibility that Brown so constructed his book in the hope that he could discuss methods of explanation without prejudice to the issue of holism-individualism. But this he has not succeeded in doing because he has not succeeded in sticking to a discussion of 'methods' of explanation only. Genetic, functional, and law explanations are separate 'methods' in some sense, although their logic is identical. But intentions, and so on, are types of explicans, nothing else. There is no 'method' of explaining by intentions; there is only explanations which contain intentional premisses.

Now since Brown has seen fit to write on the logic of explanations which contain intentional premisses, why has he not seen fit to write on explanations which contain holistic or historicistic premisses? Evading the metaphysical issue is not impossible, but once you have dabbled in the substantive content of explanations as opposed to their logic or method to the extent that Brown has, I think the metaphysical issue rears its ugly head willy-nilly.

I hope now that I have said enough to indicate that I am not criticising Brown for failing to write a different book. Rather am I insisting that within his chosen title and subject matter he has excluded discussion of certain points which strike me as vitally involved with the issues he does discuss, and germane to any conclusion he wishes to draw.

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[1] To try to prevent misunderstandings I should stress that individualism includes institutions as irreducible wholes. Individualism is *not* the view that only individuals exist; it is the view that only individual aims exist.

[2] One requirement of satisfactoriness is that the law should be independently testable. Since laws of the development of history, having no space-time reference, are not testable at all they can hardly satisfy this requirement.

Further to explain this I must come to my second point: that he utilises, but nowhere explicitly discusses, the logic of the situation. It is because he does not discuss it, I think that he nowhere takes a clear stand on the individualism-holism metaphysical issue, one of the most important issues in the foundations of social science. Yet he is evidently an individualist. Perhaps he has so far been unable to harmonise his other ' methods ' with situational logic.

The logic of the situation is at bottom a very simple and intuitive notion. We assume that people have certain aims, that they also have certain means (restricted by their physical nature and by the social set-up of institutions and traditions), and certain knowledge and beliefs about their means and about the set-up. Armed with all this they act to achieve their aims within the social situation created by traditions, institutions, and the aims and actions of other people. Such a model, of an actor facing a social set-up, equipped with certain knowledge and beliefs, and striving to attain certain ends, strikes me as being *the* explanation form of the social sciences. Combined with functionalism and the unintended consequences of actions it incorporates and can ' locate ' all of Brown's methods of explanation.

Genetic explanations are usually of the social set-up. Intentions, dispositions, and reasons refer only to individuals. Functions concern only the [illegible]cial set-up. Laws and theories can only be general—about people of a [illegible]ticular kind, and institutions, and societies of a particular kind. There [illegible]o sociological laws about individuals, as it were. By looking at these [illegible]ations in relation to the logic of the situation we can see how they all [illegible]e and can be used to supplement each other in dealing with a problem and its subsidiaries. In this way are they ' located '. The model of the logic of the situation drives from Weber and Collingwood and is referred to and relied upon in Brown's book. Yet he never says that he thinks it is basic.

Had he spent space analysing this model he need not have introduced quite so many examples. A handful, more thoroughly worked out, would have been preferable. Most books on these topics have too few examples and certainly that is worse than too many. But Brown's references to the literature of social science are so extensive that one gets perplexed. It is not easy to follow an argument when each stage of it is illustrated by a new example while the previous one is jettisoned. If it is jettisoned because it does not bring out a different aspect, then the author should say so. Not saying anything is confusing and that is why I tried to avoid this, in summarising the book, by using two cases only.

Another trouble which seems to stem from the lack of discussion of the logic of the situation, is the apparent lack of conclusion to the book's analysis. That is to say, many individual points are made and well made along the way, but the book issues in no conclusion, no problem is declared solved at the end of it. So one is left rather in the dark as to why this classification of explanations has been made. Brown himself convincingly shows that classifications lack value if they are not constructed to solve some problem. Brown's classification of explanations in social science can be seen to bear on the problem of whether social science explains at all, but refutation of the cruder views on this issue is hardly needed by social scientists. The book will do this, of course, and I have earlier said that it would also have a salutary effect on some social scientists were they to read it. But this is hardly enough. What *could* the conclusion to such a catalogue be? That there are seven, logically independent methods of explanation in social science? Brown refutes this. That the seven seemingly independent methods are interconnected? Perhaps, but this is rather weak, even when we specify the interconnections.

The conclusion I draw from the book, that logic of the situation, unintended consequences of actions, and system-maintenance are the three basic explanatory models, is also weak. Only if we concentrate on the first part of the book, where the views of those who say social science does not explain but only describes, and then historically rather than scientifically, can we see any light. Brown's problem, then, may be the perennial one of the scientific status of social science. If so, then his many examples are explained, his close-wrought arguments are explained, and his general structure is explained —not only is social science scientific in that it provides testable explanation [illegible] it also provides a sizeable number of such explanations, namely gene [illegible] intentions, dispositions, reasons, functions, laws, and theories. All of [illegible] are investigated in Brown's book to show under what conditions th [illegible] explanations and under what conditions testable explanations and t [illegible] extent explanations of each other.

I. C. J[illegible]

COLLINGWOOD RECONSIDERED

In writing about Collingwood one has an option similar in some respects to that enjoyed by Nietzsche scholars: one may look for either contradictions or continuities, confident of finding plenty of what one is seeking. The search for continuities is, however, the more fruitful approach, a fact which has already been demonstrated in Nietzsche scholarship and which is now established by Donagan in his very good study of Collingwood.[1] One other parallel with Nietzsche studies may be cited: just as some scholars have been tempted to explain or dismiss Nietzsche's later work as the fruit of insanity so others are tempted to explain or dismiss some of Collingwood's later judgments as the products of ill health. Donagan has resisted this Knoxian temptation. He is to be congratulated for it and for having made as good a case for the later Collingwood as seems possible, even in matters where he himself admits to finding Collingwood unsatisfactory. Ordinarily, however, Donagan finds Collingwood more praiseworthy than not; and his sympathetic and skilful exegesis will, I hope, have the effect of furthering serious reconsideration of some of Collingwood's views. His book will perhaps be most effective where the philosophy of history is concerned and least effective in the areas of 'mind and language' and of aesthetics. I shall discuss only Donagan's restatement of Collingwood's philosophy of history, assuming that readers will be most interested in the question of whether history is a science.

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[1] *The Later Philosophy of R. G. Collingwood.* By Alan Donagan. Clarendon Press, Oxford, 1962. Pp. xi+332. 42s.

The question of whether history is a science is often a concealed version of another question, Is knowledge of the past really possible? As such it is a reflection of the modern suspicion that apart from mathematics and the natural sciences there is no genuine knowledge. For Collingwood, however, the question of whether history is a science emerged from an altogether different climate of opinion, one in which the predominant question was whether the findings of the natural scientist were to count as knowledge at all. The early Collingwood took the position that scientific thinking because it is abstract is necessarily false; and while the later Collingwood came to see the absurdity of this view and to side, belatedly, with the angels, the positivists, on this issue, the question of whether history is a science was never for him a question of whether we should honour the historian by deciding that he is after all a scientist (although an inexact one to be sure).

I think nothing irritated Collingwood so much as the judgment that history is an inexact science, which was, he felt, wrong on two counts. First, historical knowledge can be as precise and as certain as anything offered by the natural sciences; second, it is incorrect to rank the historian as a second class scientist. History to Collingwood is *sui generis*, and he is remembered for his Aristotelian search for that which is characteristically historical both as to subject matter and method. It would, however, be inaccurate to see Collingwood as intent only upon marking off history from the sciences. Instead the later Collingwood insisted that history and the natural sciences share in a general way the Baconian method of 'putting nature to the question'. Yet there is, Collingwood thought, a great difference between an event in nature and a human action; consequently within the Baconian methodology common to the sciences and history there must also be significant differences. Scientists and historians ask different questions and proceed with different ends in view. The question 'Why did that happen?' invites answers different in kind when asked of an eclipse and when asked of Caesar's invasion of Britain. In the first case the scientist is interested primarily in subsuming an event under a general law; in the second case this is not true of the historian who is interested in the thoughts and intentions Caesar had in mind when he acted as he did. To get at these, Collingwood maintained, the historian must rethink the thoughts of the historical agent and reconstruct his picture of the situation which confronted him.

Such in brief is Collingwood's way of marking the resemblances and differences between history and the sciences. Does it justify his conviction that history is not one of the sciences but that it can nevertheless supply us with knowledge at least as reliable as that provided by the sciences ? Donagan thinks it does, while I think it does not. If, as Collingwood contended in *The Idea of History*, the historian need not and cannot in his professional capacity emulate the scientist in searching for the laws of events, then how does the historian connect his hypotheses with his evidence? Collingwood's own highly self-conscious activities as an historian are relevant to this question, and Donagan has wisely chosen to consider Collingwood's treatment of Caesar's invasions of Britain as being especially revealing (pp. 182-192).

The question Collingwood set himself arises from the fact that Caesar twice invaded Britain and twice withdrew his forces. Did he plan these withdrawals from the very beginning? Were his expeditions punitive or demonstrations of force? The only evidence lies in Caesar's *Commentaries*. Collingwood maintained that if the invasions had been punitive in intention, Caesar would have said so in the *Commentaries*. But he did not; therefore, they were not. Moreover, while he does not state in the *Commentaries* what his intentions were, we know from the *Commentaries* that Caesar's invading forces were quite large. We have no reason to believe that Caesar was an invincible soldier or a wholly candid reporter of his own motives and intentions. Hence Collingwood reasoned that Caesar had actually intended to subdue at least a large portion of Britain, that he had failed, and that he deliberately did not mention his original intention in the *Commentaries* so that no one should know of his failure. Collingwood thought that this hypothesis was as respectable as those offered in the various sciences, that it fitted the facts better than any alternative hypothesis, and that it did not rest upon general laws.

Let us consider Collingwood's attempted refutation of the hypothesis that Caesar's expeditions were successful punitive undertakings. Where, Donagan asks, are there any general laws either present in or presupposed by Collingwood's argument? Certainly the categorical premiss (' Caesar did not make plain in his *Commentaries* what he had intended ') is not a general law. Nor, thinks Donagan, is the hypothetical premiss (' If Caesar's invasion had been a successful punitive expedition, and if he had written his *Commentaries* to advertise his successes and had known that in order to advertise e success of his invasion (if it had been a success) it would have been necessary ake plain in his *Commentaries* what he had intended'). This cannot be a g aw, Donagan asserts, because it is about a single individual. Does it pr ose a general law? No, but it does presuppose a general hypothetical 'about anybody and everybody'. It is not a general law but an analytic truth which is presupposed, 'an analytic truth which derives from the very concept of an intention': 'No conceivable empirical evidence would count against the proposition that if you hold to your intention to bring something about, and believe that you must take certain steps to do so, then you will take those steps if you can. Any evidence which goes to show that you held that belief but did not take those steps, although you could have, also goes to show that you did not stick to your intention' (p. 185). Thus Collingwood is able, Donagan thinks, to get from his categorical premiss and his hypothetical premiss to his conclusion ('So, since Ceasar did write his *Commentaries* to advertise his successes, and did know that in order to advertise the success of his invasion it would have been necessary to make his intention plain, his invasion was not a successful punitive expedition') without resort to general laws. Not all hypotheticals employed by historians are analytic truths about what people with certain intentions will do in certain situations, Donagan concedes; but when they are not they are derived from independently verifiable propositions about what historical gents think.

The cleverness of Collingwood's interpretation of Caesar's invasions ritain is beyond doubt. Two questions, however, persist: (1) Is it to as knowledge, as being as reliable as the hypotheses offered by the ? (2) Does it show, as Donagan believes, the autonomy of history? Collingwood's interpretation is acceptable is best left to Roman , but their verdict seems to be that the question of Caesar's intentions remains an open one. Given the nature of the evidence, or more accurately the lack of evidence, this seems a reasonable professional attitude. As to whether Collingwood's conjecture is as reliable as the hypotheses offered by the sciences, it is on the face of it absurd to argue with assurance about the intentions of an historical agent when the evidence is either nonexistent or exceedingly indirect and then to complete the bluff by saying, Well, this is as good as anything the sciences can offer. Surely there are better examples of sound historical hypotheses available for any friend of history to use. It was not, however, by accident, I think, that Collingwood mistook a bad example for a good one; it was rather a reflection of his conviction that the historian can by 'putting himself in the agent's place' and 'rethinking' his thoughts attain certainty as to what the agent was about. To be sure, Collingwood usually qualified or supplemented this, as Donagan reminds us, by emphasising the need for critical scrutiny of the evidence; but there is an obvious tension within his philosophy of history between the empathic and critical methods. It was not the early but the later Collingwood who wrote (in the *New Leviathan*, 14.22) that knowledge is 'the conviction or assurance with which a man reaffirms a proposition he has already made after reflecting on the process of making it and satisfying himself that it is well and truly made'. Donagan sees this as being badly mistaken, but he fails to connect this error with the main drift of Collingwood's thought or to regard it as a consequence of Collingwood's 'put yourself in his shoes' theory.

The second question of whether Collingwood's treatment of Caesar's invasions demonstrates the autonomy of history has already been touched upon. In its positive aspect, the autonomy of history is revealed, according to Collingwood, by the historian's rethinking or reconstructing the thoughts of historical agents; in its negative aspect, it is shown by the absence of general laws. To be sure, history is not wholly autonomous from the sciences; it may sometimes employ scientific techniques, radio-carbon dating for example, but only in an ancillary capacity. Even if the social sciences could provide general laws about how invading generals think or behave, this would, Collingwood believed, be wholly irrelevant to the historian's explanation of why *Caesar* thought or behaved as he did. It is no help in explaining why X did Y in situation Z to be told that people like X always or often do Y in Z-situations.

Clearly what is involved here is the question of what is to count as a
explanation in history. Covering law theorists such as Hume, Mill, a
Hempel hold that an event is not explained until it is deducible from ge
laws conjoined with statements about initial conditions; Collingwoo
Donagan while conceding that this is true of the natural sciences de
it is true of history. The hypotheticals employed by historians are, D
asserts in his analysis of Collingwood's Caesar study, either
truths about what people with certain intentions do in certain situ
else they are derived from independently verifiable propositions ab
thoughts of historical agents. Concerning Collingwood's hypoth
premiss in his attempted refutation of the idea that Caesar's invasions w
of a punitive nature, Donagan states that this could not be a general la
because it is about a single individual, Caesar. Donagan concedes, however, that in the sciences 'the application of some general laws appears to be confined to certain regions of space, e.g. the law that on the earth's surface at sea-level the boiling-point of water is 100 degrees C.; but such laws are covertly general' (p. 186). The covering law theorist would most certainly reply that laws which appear to be confined to a certain person are also 'covertly' general, that is, they are applicable to any persons sufficiently like Caesar who might be in sufficiently similar situations; and he would deny that the number of instances of a law is relevant to the question of whether a given 'law' is really a law.

Does Collingwood's hypothetical premiss, as Donagan asserts, presuppose simply an analytic truth 'about anybody and everybody', a truth which derives from the concept of 'intention'? What is to count as an analytic truth about the concept of intention is, however, far from clear, for two reasons: (1) after Quine the analytic-synthetic distinction is not in the best of health, (2) the problem of intentions is such that it is an anticipation of some future comprehensive analysis to say with certainty what does and does not follow from the concept of 'intention'. Donagan's argument seems to rest upon the following consideration: to say of someone that he has an intention seems stronger than to say that he has a wish. A person who intends to do X seems more committed, given the chance, to doing X than a person who might merely wish to do X. If he intends to do X and believes that he must do Y to bring about X, it follows that he will, if he can, do Y. If Caesar intended to advertise his military successes and believed that he had to make his intentions concerning Britain evident before his success (if any) would be apparent, then he would have made clear (if he could have) what his intentions concerning Britain actually were. Collingwood's argument does presuppose that people with certain intentions will do certain things, perform certain acts, in certain situations, if they believe these acts will help in the realisation of their intentions; but is this an analytic truth about 'intentions' or a synthetic truth about how people who have certain intentions do in fact behave?

A key to the controversy lies in a footnote where Donagan recognises [illegible] his analysis of 'counter-factual partly closed hypotheticals will not do [illegible] hose hypotheticals which are assumed to be derivable from laws; but [illegible] sumption need not be made' (p. 189). Since the problem of counter- [illegible] like that of intentions, also ranks high on the list of unresolved [illegible], dogmatism is out of place here; but given the work done in this [illegible] uld seem that the burden of proof falls upon Donagan to show why [illegible] sumption' need not be made in the case of the hypotheticals em- [illegible] by historians. And this he does not do.

[illegible] overing law theorists are sometimes accused of indifference to the [illegible] exts in which explanations are offered, an indifference which leads them to neglect the almost bewildering variety of explanations which are used and used successfully in ordinary life. Since the covering law theorist is usually concerned with only one context, the scientific, what right has he to assume that scientific explanation wears the trousers for all explanations? The answer is that he doesn't, or at least needn't. All he is saying is that if an explanation is to count as scientific, it must be of a certain form. Against the alleged dogmatism of the covering law theorist there is, however, posited a greater dogmatism, one which asserts that an action can *always* be explained adequately in terms of an agent's reasons or thoughts without recourse to general laws. Hence the hostility of most opponents of the covering law model to the various social sciences, especially psychology. While Donagan takes pains to defend psychology against some of Collingwood's more outrageous judgments, he approves of Collingwood's belief that the resemblances between the social sciences and the natural sciences are superficial and attempts to back up Collingwood with Popper's distinction between *bona fide* laws and pseudo-laws which are really statements about trends or patterns of behaviour. Donagan quotes with approval Collingwood's verdict that 'A positive science of mind will, no doubt, be able to establish uniformities and recurrences, but it can have no guarantee that the laws it establishes will hold good beyond the historical period from which its facts are drawn' (*The Idea of History*, pp. 223-4). To this Donagan adds: 'Because events in contemporary history resemble portions of some pigeon-holed groups of past trends, it does not follow that their sequel will accord with those trends' (p. 236). The answer to this seems to be that, of course, it does not follow in any deductive sense of 'follow', but in what other sense does it not follow? Of course, there is no ironclad guarantee that the laws of psychology will hold good beyond the historical period from which they are drawn; but surely psychology, physics, and all the inductive sciences are in the same boat in this respect, however they might differ in others.

The efficacy of history as a species of practical wisdom, as championed by both Collingwood and Donagan, seems to depend upon our being able to spot resemblances between contemporary events and past ones and to ant[illegible]cipate, in however rough and ready a way, the future course of events [illegible] certain kinds of situations. Donagan later remarks that Collingwood [illegible] 'oblivious' of the fact that on his own analysis history can 'yield ready [illegible] rules of a kind. Historical situations recur, just as natural situati[illegible] (p. 239). Although Donagan would deny it, this admission [illegible] considerably the gap between Collingwoodians and covering law [illegible] What good would rules of practical conduct be unless these rules [illegible] posed regularities in human behaviour?

BURLEIGH TAYLOR WILK[illegible]

The Myth of Metaphor. By Colin Murray Turbayne.
Yale University Press, 1962. Pp. ix+224. $6.00.

THROUGHOUT the history of science there have been at least two w[illegible] looking at a scientific theory, either as a description of the facts, or as an hypothesis about them. This book endeavours to give a new slant to the latter approach, one more in keeping with contemporary linguistic philosophy. Turbayne's *bête noire* is the classical mechanical picture of the universe. To this he opposes Berkeley's view that the laws of nature form a language, which he believes to be preferable, especially when expressed in an *as if* fashion. Turbayne quotes with approval Cardinal Bellarmine's advice to Galileo respecting the heliocentric theory. ' Galileo will act prudently if he will speak hypothetically ' (p. 40).

Turbayne's main contention is that physical theories whether expressed in classical mechanical form or linguistically, are to be regarded as metaphors. He, however, extends the notion of metaphor beyond the usual dictionary meaning. His use of it has more in common with Ryle's definition of the category mistake, namely, ' It represents the facts . . . *as if* they belonged to one logical type or category . . ., when they actually belong to another ' (p. 17). This definition, he says, brings out the make-believe *as if* character of such theories. Both Descartes and Newton, for example, although they assumed they were giving true descriptions of the world, approached it metaphorically. What they were really doing was making hypotheses and projecting them on to nature. The mechanical model which they used, we are told, is ultimately based on the more fundamental model of Euclidean geometry, with its two essential properties of deduction and extension. When motion is added, this model becomes transformed into the mechanical one. Descartes applied it to optics, magnetism, biology, the human body and only stopped short of the soul.

Turbayne believes that a more useful account of the events of nature can be given, if they are considered as composing a universal language rather than giant clock-work. He compares the vocabulary of the mechanical model [illegible]ch consists of parts, laws of operation, etc., with that of the linguistic [illegible]l consisting of signs of things signified, rules of grammar, etc. Both these models start with the same facts which they take as descriptive of the physical world. In the former they are interpreted as the effects of efficient causes hidden in the works of the machine—moving of necessity. In the latter they are interpreted as signs of a language which suggest things signified —these signs are tied to the world. Though preferring the linguistic model, he sees no intrinsic reason for choosing between them. He regards them both as two different ways of speaking about the same fact, or two different pictures of the same subject.

I find a number of difficulties with Turbayne's position. In the first place, if Galileo and his successors had taken Bellarmine's advice and spoken hypothetically, it is questionable whether astronomy and physics would have advanced very far. Further, Turbayne's use of metaphor differs, as he clearly recognises, from its normal usage. If we call a man a lion, we are not confusing logical types, but speaking figuratively. We are consciously attributing some of the qualities of the one to the other. Much of scientific theorising involves the use of analogical procedures. In asserting that the mechanical and linguistic models are really two different ways of speaking about the same facts, Turbayne would seem to assume something like a phenomenalism; that the physicist's account of the world is reducible to a routine of sense-data. Whereas in the mechanical model we introduce scientific objects, in the linguistic model we presumably start with sense-data, for which our linguistic terms are to be regarded as signs. However, without the mechanical model in the first place it would not have been possible to express these laws of nature in a linguistic or sense-data form. Indeed Berkeley's view that the laws of nature are the language of God, merely translates the laws of nature as formulated by Kepler, Galileo and Newton into sensory terms; their regulation being handed over to the Deity. What Berkeley is then really doing is a form of coding, in which physical statements expressed in geometrical terms are translated into routines of sense-data.

A further criticism might be made. If our physical theories about nature are just metaphors, how is it that they have been so strikingly successful in the past in enabling us to exercise some control over nature? It is partly because of this that we assume that our physical theories have some common causal structure with the world. Consider geometry: physical objects have geometrical characteristics, i.e. shape, size, etc., although they may not exhibit them as precisely as they appear in geometry books. There seems nothing arbitrary about the fact that some kind of geometry is applicable to the world around us, still less are we using metaphors when we speak of things as having geometrical properties. Turbayne nevertheless has a point here. Although there may be good reasons for using the mechanic model in physics, its extension to such fields as biology and the social scien may be unjustified.

As for scientific objects, are they too essentially metaphorical in n Quite a large number of scientists including the late Lord Rutherford firmly believed in the reality of electrons and other scientific objects. ther, analogy has been important in the advance of science, models bei taken from one field and applied to another. In this way science has cross-fertilised itself. Quite often behind the analogy there is an implicit appeal to some principle underlying both the model and the new data to which it is applied. Such a principle may only be dimly perceived when the model is first applied to this field. Is it because Turbayne implicitly assumes that there is only one kind of reality, namely, that of our perceptions, that he accuses scientists of indulging in metaphor? But even perceptual objects are not free from interpretative elements. Are they also forms of make-believe?

The second half of this book is devoted to the problem of vision. Turbayne tells us that most writers on optics both ancient and modern have assumed that optics falls under geometry. The consequences of the geometrical theory of vision are to be found, he says, in the representative theory of perception which is central to seventeenth- and eighteenth-century epistemology. This assumes that what is true of the camera is true of the eye; that the eye is a camera-machine for taking pictures of the external world.

Just as the images on the back of the camera may be obscure and confused or clear and distinct, so there are similar kinds of ideas in the mind. Taking Berkeley once again as his guide, Turbayne believes that the linguistic model has the advantage over the visual geometrical model in explaining such phenomena as illusions, the relativity of perception, etc. Among the problems on which he believes light may be thrown are those of perceived distance, the apparent size of the moon and the inverted retinal image. He also believes that these problems form a test case by which we may judge the superiority of the linguistic model over the mechanical model.

It is difficult to distinguish cause from effect here. In the first instance knowledge of the actual structure of the lens of the eye probably stimulated the study of artificial lenses, and with it the telescope, microscope and the camera as we know it today. It might indeed be said that the machine model of nature and the camera model of vision, have their analogues in the various parts of the human body. Just as the camera is an extension of the eye, so are, for example, levers extensions of our limbs. It is true, however, that psychologists and biologists have in their descriptions of psychological and biological phenomena, tended to use models taken from the physical sciences in order to make their accounts more objective. In this way they may sometimes have distorted their data.

Turbayne's position as regards a visual language of nature is substantially
e same as Berkeley's, namely, that the relation of cause and effect is that of
g seen to thing signified, and that we are enabled to infer one from the
by a process of arbitrary association. In thinking of distance, for
le, we really argue from the size as we see it, to size as we feel it. It is
l, however whether the relationship between visual and tactual data is
bitrary. Consider the case of depth, we certainly see depth, although
by implicitly accepting the view that the retinal image is two-dimensional argues otherwise. Tactual data are brought in then to explain why we nevertheless see things three-dimensionally. Or take the case of the moon which looks larger near the horizon than at the meridian, although the angle under which it is seen is not greater. This is explained by saying that although the visual signs, in this case the moon in its different aspects, remain physically the same, they yet suggest different things, i.e. different tactual data. The root of Berkeley's difficulty lies in the fact that he endeavours to correlate the dimensions of the physical stimulus on the retina with those of its effects, namely the visual sensations. To explain why the latter vary though the stimulus remains constant he has to bring in past tactual associations. In the case of the inverted retinal image he gets over the difficulty that we nevertheless see things right side up, by adopting as his criterion of reality tactual rather than visual data. But the ordinary person who knows nothing of what occurs on the retina finds no such difficulty until the physiologist puts him wise.

In the light of the above we may doubt whether the illusions of vision form a test case for the superiority of the linguistic model over the mechanical model. Further, Turbayne overlooks that ordinary language is not always a useful guide in dealing with the abstractions of science, and that it may have difficulty in coping with some of the conceptual innovations in physics and elsewhere. Not all technical terms can be adequately translated into ordinary language. Nevertheless, this is in some ways a stimulating book. Turbayne's discussion of the theory of geometrical vision is much more interesting and constructive than his discussion of metaphor. He has done some hard thinking about this topic, and he has also looked at it in its historical perspective.

W. Mays

ANNOUNCEMENTS

EIGHTH ANNUAL CONFERENCE OF THE BRITISH SOCIETY FOR THE PHILOSOPHY OF SCIENCE

The Eighth Annual Conference of the British Society for the Philosophy of Science was held at St Hugh's College, Oxford, 20th to 22nd September 1963. It comprised a colloquium of four symposia and was attended by some sixty members and guests. Mr F. T. C. Harris of the Middlesex Hospital Medical School was Conference Secretary.

The four Symposia were entitled 'Models of Biological Growth Processes', '*A Priori* and Empirical in Economics', 'Value Judgments in Economics', and 'Mathematical logic in Relation to the Philosophy of Science'.

The chairman of the first symposium was Professor D. R. Newth of the Middlesex Hospital Medical School. Dr H. Kacser, Edinburgh gave the opening paper entitled 'Causality, Complexity, and Growth'. Dr Kacser chose examples from his own field of embryology to support his argument that it is at least as important to achieve understanding as to make predictions. The mechanism of growth involves a system of chemical kinetics in which processes are dependent on many variables. The nvestigation of such growth mechanisms involves (i) the identification of the elements [illegible] analysis, organs, molecules, etc., (ii) the establishment of a functional relationship ween the elements, and (iii) the 'synthesis' of the organism in terms of this sis. However, as it is not possible to apply the second of the above procedures iguously to multi-variant systems, it is thus in principle impossible to make ns. A causal explanation of growth in terms of chemical kinetics, *ceteris* [illegible] in practice trivial. The characteristics of the chemical kinetic growth system are just such that other variables never are equal. Dr Kacser held that the approach followed by his own unit, of simulating enzyme controlled processes in terms of differential equations and of computing these sets of equations in a matrix computation would lead to comprehension of the mechanisms influencing growth processes. Mr F. T. C. Harris, London, then gave a paper entitled 'Description, Simplicity, and Growth', whose subject fell into the first of Dr Kacser's three categories, the identification of the elements of analysis. Whatever the desirability of considering numerous contingent factors in causal explanation, Mr Harris held that simplicity was essential at the level of description. In picturing morphogenesis embryologists have in mind a norm or ideal developmental pattern to which their material largely conforms and whose part in embryological theory is analogous to that played in statistics by the concept of an infinite population. Simple descriptive concepts such as cell and number were shown to provide models of morphogenetic processes based on ideal developmental patterns. The models took the form of sequences of numbers which provided a classification of mosaic cleavage patterns that was more concise than that at present current. A specific example of the description of the cleavage of a sea urchin was used to illustrate the principles involved in formulating the models and in their empirical derivation. It was pointed out that the principles employed provide a technique for seeking wider generalisations than are at present available. Further, these generalisations have the feature of being purely biological in that, at a descriptive level, they involve no reference to the conceptual apparatus of chemistry or of physics.

The chairman for the second symposium was Professor G. H. L. Shackle, Liverpool. Professor Richard Stone, Cambridge, gave the opening paper in which he illustrated by several examples the way in which the *a priori* and the empirical are intertwined in economic analysis and argued that it did not matter whether one started with theory or with observations as long as one did not remain at one's starting point but passed continually from one to the other. In this talk he concentrated on the analysis of consumer's behaviour. Miss Anne Martin, Oxford, followed. She held that most economists are unclear as to whether their basic principles are testable or *a priori*, and fail to preserve clearly the distinction between analytic and synthetic propositions. She considered that *a priori* knowledge is sometimes equated with introspectionist knowledge of ourselves, and she dwelt on some of the unfortunate consequences that arise from this.

Mr J. W. N. Watkins, The London School of Economics, was chairman at the third symposium. Professor R. L. Meek, Leicester, gave the opening paper. He explained that most early economic thinkers invoked political and theological criteria in judging economic policies. By the late eighteenth century economists conceived their task as the analysis of the laws governing the operation of a kind of giant economic machine. Nevertheless, value judgments have been imported subsequently into economic analysis. In the new economics developed since the last war, value judgments will undoubtedly enter but will not play much part. Mr K. Klappholz, of th London School of Economics, gave the second paper. He considered the questi whether economics can be ' wertfrei ' or whether it is necessarily ' value-impregnat and held the claims made for the idea of ' value-impregnation ' have no bearin ' wertfrei ' in the usual sense. The paper briefly examined the problem o personal comparisons of utility and its relation to value judgments, and the pr value-free, i.e. hypothetically-imperative advice. In conclusion it was urg widespread desire to exclude normative questions from academic economi is misguided. The four economics papers will appear in this *Journal* in t Number.

Professor W. C. Kneale was chairman of the final symposium. Dr Kneebone, Bedford College, London, opened. According to him, whereas philosophers of mathematics have treated formal logic as simply an instrument serves to make explicit the deductive structure of mathematical theories, others h attempted to reduce mathematical concepts themselves to purely logical terms; b although the first of these ways of applying a logical calculus has now been largel adopted by the mathematical world, the second has not commended itself to mathematicians. In the philosophy of science, the place of logical calculi is still more restricted. The testing of scientific hypotheses is facilitated and made more efficient by axiomatisation of the theories in which the hypotheses occur, and formalization of theories is accordingly desirable. But the development of science in the large is a dialectical process, involving evolution of concepts and replacement of existing theories by more adequate ones; and the role of deductive logic in natural science is of necessity purely instrumental. Dr M. H. Löb, Leeds, pointed out that mathematical logic is one of the most active areas of mathematical research and that fruitful pursuit of the philosophy of mathematics necessitates constant reference to technical work in logic, because many concepts relevant to investigations in the foundations of mathematics have been explained in formal terms. Thus not only has formal logical analysis replaced the philosophy of mathematics but it is similarly applicable to the philosophy of science.

F. T. C. Harris
D. G. Harris

The Ninth Annual Conference of the British Society for the Philosophy of Science will be held at Churchill Hall, Bristol University, from Friday 18th to Sunday 20th September 1964. The programme is being arranged in the form of four symposia:

Friday Evening—'Rationality'
Chairman Dr J. O. Wisdom
Speakers Mr Jonathan Bennet and Dr W. W. Bartley

Saturday Morning—'Corroboration'
Chairman Professor S. Körner
Speakers Mr L. J. Cohen and Dr J. L. Watling

Saturday Evening—'Causality'
Speakers Dr P. Alexander and Mr A. J. Lyon

Sunday Morning—'Decision'
Speakers Professor G. L. S. Shackle and Professor G. P. Meredith

The Conference Fee will be £1. The full charge for the conference will be £5 and includes gratuities and the conference fee. Participants staying shorter periods will be charged pro rata for accommodation and meals. Enquiries and bookings should be sent to Professor S. Körner, Department of Philosophy, The University of Bristol. Cheques should be crossed and made payable to the British Society for the Philosophy of Science.

1964 International Congress for Logic, Methodology and Philosophy of Science

The Hebrew University of Jerusalem, Jerusalem, Israel

26 August—2 September, 1964

An International Congress for Logic, Methodology, and Philosophy of Science will be held at The Hebrew University, Jerusalem, Israel, from 26 August to 2 September, 1964, under the joint auspices of the International Union for History and Philosophy of Science and the Israel Academy of Sciences and Humanities.

The Congress will be organised into nine sections. Each section will consist of a one-hour invited address, a $2\frac{1}{2}$-hour symposium with three of four participants and a number of contributed twelve-minute papers. The first two sections will, in addition, contain a limited number of invited half-hour addresses.

ANNOUNCEMENTS

This is the preliminary list of the nine sections, their chairmen, the invited one-hour and half-hour speakers, and the participants in the symposia. (A number of invitees—with an asterisk after the name—have not yet finally accepted the invitations so that certain minor changes might be expected.)

Section 1: Mathematical Logic; A. Mostowski (Poland), chairman.

One-hour address:

Symposium on *Applications of the Theory of Mathematical Machines to Mathematical Logic.*

R. J. Buechi (USA), A. Ehrenfeucht* (Poland), W. P. Hanf* (USA), A. A. Markov* (USSR), J. C. Shepherdson (England).

Half-hour addresses: A. Cobham (USA), M. O. Rabin (Israel), R. L. Vaught (USA).

Section 2: Foundations of Mathematical Theories; A. Tarski (USA), chairman.

One-hour address: A. I. Malcev (USSR), ' On Metamathematics of Algebra '.

Symposium on *Foundations of Set Theory.*

C. C. Chang (USA), P. J. Cohen* (USA), A. Levy (Israel), R. Montague* (USA).

Half-hour addresses: H. J. Keisler (USA), S. C. Kleene (USA), W. Schwabhauser* (East Germany).

Section 3: Philosophy of Logic and Mathematics; P. Lorenzen (West-Germany), chairman.

One-hour address:

Symposium on *The Justification of Formalization.*

S. Körner (England), A. Robinson (USA), —.

Section 4: General Problems of Methodology and Philosophy of Science; I. Scheffler (USA), chairman.

One-hour address: M. Polanyi (England), ' Science; Tacit and Explicit '.

Symposium on *Logical and Pragmatical Aspects of Explanation and Prediction.*

Mary B. Hesse (England), B. Mandelbrot (France), W. Sellars (USA), M. White (USA).

Section 5: Foundations of Probability and Induction; G. H. von Wright (Finland, chairman.

One-hour address: M. Black (USA), ' Probability and Rational Belief '.

Symposium on *Probability and Rational Belief.*

R. B. Braithwaite (England), H. Kyburg (USA), —.

Section 6: Methodology and Philosophy of Physical Sciences; A. Grünbaum (USA), chairman.

One-hour address: O. Costa de Beauregard (France), ' Irreversibility Problems '.

Symposium on *Space, Time and Relativity.*

P. Bergmann (USA), P. Havas (USA), H. Mehlberg (USA).

Section 7: Methodology and Philosophy of Life Sciences; P. Suppes (USA), chairman.

One-hour address:

Symposium on *Concept Formation.*

P. Ya. Galperin* (USSR), W. McCulloch (USA), S. Papert (England), F. Restle (USA).

Section 8: Methodology and Philosophy of Historical Sciences; S. Toulmin (England), chairman.

One-hour address: R. Aron (France), 'Can there be a Critique of Historical Reason?'

Symposium on *Evidence and Theory in the Historical Sciences.*

S. Dedijer (Yugoslavia), E. Nagel* (USA), —.

Section 9: History of Logic and Methodology of Science; W. Kneale (England), chairman.

One-hour address: A. Church*, 'The History of the Question of Existential Import'.

Symposium on *Selected Problems in Greek and Medieval Logic.*

P. Geach (England), N. Rescher (USA), G. Paetzig (West Germany).

The published Proceedings will contain the invited lectures, the contributions to the symposia, and the programme of the Congress.

The fee for membership in the Congress is $5.00; for associate membership—$3.00. Members of the Congress are entitled, upon request, to receive the Proceedings at a reduced rate of $12.50 (with the regular rate being $18.00). Please make cheques or bank drafts payable to the International Congress for Logic, Methodology, and Philosophy of Science.

Members of the Congress are entitled to participate in the meetings of the International Colloquium on Algebraic Linguistics and Automata Theory that will convene in the same place the two preceding days, i.e. 24-25 August, 1964.

Peltours, 28 Ahad Ha'am Street, Tel Aviv, have been appointed official tourist and travel agents to the Congress and will, through their offices and agents overseas, approach you in the near future offering their service in arranging your travel and accommodation in Israel during the period of the Congress, and they will also be able to provide any additional information required by you.

Further information can be obtained from the Congress secretariat.

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A misprint occurs in John Padel's review of *The Foundations of Science and the Concepts of Psychology and Psycho-Analysis*, edited by H. Feigl and M. Scriven (this *Journal*, 1964, 14, 356-9). On p. 357, last line, for 'perpetual' read 'perceptual'.

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Bayle Not a Sceptic ?. . . . . *Harry M. Bracken*
Immaterialism in Jonathan Edwards' Early Philosophical Notes . *Wallace E. Anderson*
Tocqueville's Two *Democraties* . . . . *Seymour Drescher*
Mill and Tocqueville . . . . *H. O. Pappé*
Mill's History of His Ideas . . . . *Robert D. Cumming*
The Writing of American Histories of Ideas:
Two Traditions in the XXth Century . . . . *Robert Allen Skotheim*

NOTES

Chaucer's Characterization of the Canon and His Yeoman . . *Joseph E. Grennen*
Joseph Priestley, The Theory of Oxidation and the Nature of Matter *Robert E. Schofield*
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PAUL LORENZEN	*Metamathematik*—Bibliographisches Institut Hochschultaschenbucher Vol. 25 (W. Ackermann)
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Is Existence a Predicate ?	MURRAY KITELEY
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The Terms and Sentences of Empirical Science	G. SCHLESINGER
Moral and Religious Conceptions of Duty: An Analysis	D. Z. PHILLIPS
Discussions:	
Primary and Secondary Qualities	AAGE SLOMANN
Plato, Parmenides, Ryle and Exemplification	IRVING BLOCK
My Motive and its Reason	DONALD S. MANNISON
The Third Possibility	B. MELTZER
Falsifiability of Scientific Theories	R. G. SWINBURNE
Berkeley's Theory of Meaning	E. J. FURLONG
Note on Father Owens' Comment on Williams' Criticism of Aquinas on Infinite Regress	J. F. M. HUNTER
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Garrett L. Vander Veer	*Austin on Perception*
Francis J. Coleman	*Liking and Approving of a Work of Art*
CRITICAL STUDY	
Karsten Harries	*The Gnoseo-Ontological Circle and the End of Ontology*
EXPLORATION	
H. J. McCloskey	*Some Concepts of Cause*
DISCUSSIONS	
Charles Hartshorne	*What the Ontological Proof Does Not Do*
John Wild	Being and Time: *A Reply*
Robert C. Neville	Ehman's Idealism

BOOKS RECEIVED

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ANNOUNCEMENTS

INDEX

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SEPTEMBER 1964, VOL. XXV, No. 1

CONTENTS

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The Principle of Sufficient Agreement, by BERTRAM MORRIS
Reflections About Total Views, by ARNE NAESS
Epoché and Fable in Descartes, by DONALD B. KUSPIT
Transcendental Phenomenology and Existentialism, by JAMES M. EDIE
The Concept of Estrangement in the Early and Later Writings of Karl Marx, by JOHN O'NIELL
Problems and Requirements in Theory Rendering, by PAUL WEINGARTNER
Ordinary Experience, by A. H. JOHNSON
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TABLE OF CONTENTS

ARTICLES

Gerson and the Right of Resistance (1642-1644) . . . . . . *Zofia Rueger*
Science in Early Victorian Thought . . . . . . *Walter F. Cannon*
Historicism in Dilthey, Troeltsch, and Meinecke . . . . . *Calvin G. Rand*
T. G. Masaryk's Revision of Marxism . . . . *Erazim Kohak*
Peirce's Debt to F. E. Abbot . . . . *Daniel D. O'Connor*

NOTES

Social Roots of Heracliteanism . . . . *Bertrand Helm*
Yaha Ibn 'Adi's Treatise on Logic . . . *Nicholas Rescher* and *Fadleu Shehadi*
Racism and the Western Tradition . . . . *Dante A. Puzzo*
Philosophy and History . . . . *Frank H. Knight*

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CONTENTS

ARTICLES

VALUE-JUDGMENTS IN ECONOMICS 89
by Ronald L. Meek

VALUE JUDGMENTS AND ECONOMICS 97
by Kurt Klappholz

THE A PRIORI AND THE EMPIRICAL IN ECONOMICS 115
by Richard Stone

EMPIRICAL AND A PRIORI IN ECONOMICS 123
by Anne Martin

DISCUSSIONS

ESP AND THE 'THEORY OF RESONANCE' 137
by C. T. K. Chari

NOTE ON GOODSTEIN'S 'THE SIGNIFICANCE OF INCOMPLETENESS THEOREMS' 140
by Arthur I. Fine

REPLY TO MR FINE'S NOTE 141
by R. L. Goodstein

REPLY TO G. A. BARNARD 141
by R. J. Diamond

CONTENTS—(*contd.*)

REVIEWS

The Revolution in Anthropology 143
article-review by Robert Brown

Scientism and Values. Ed. H. Schoek and J. W. Wiggins
Relativism and the Study of Man. Ed. H. Schoek and J. W. Wiggins 151
reviewed by I. C. Jarvie

The Structure of Scientific Revolutions. By Thomas S. Kuhn 158
reviewed by H. V. Stopes-Roe

Scientific Explanation, Space and Time. Ed. H. Feigl and G. Maxwell 161
reviewed by R. Brown

Foresight and Understanding. By Stephen Toulmin 164
reviewed by John Watling

Wittgenstein und die moderne Philosophie. By Justus Hartnack 166
reviewed by Eva Cassirer

The Philosophical Impact of Contemporary Physics. By Milič Čapek 168
reviewed by Mario Bunge

RECENT PUBLICATIONS ON THE PHILOSOPHY OF SCIENCE 171

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The British Journal for the Philosophy of Science

VOLUME XV August, 1964 No. 58

VALUE-JUDGMENTS IN ECONOMICS *

RONALD L. MEEK

WHEN the new science of political economy burst upon the world in the eighteenth century, one of the things about it which most impressed observers was that economic thinking seemed to have largely divorced itself from value-judgments. Some people approved of this; others were horrified at it; very few failed to notice that a revolutionary change of attitude had taken place. What exactly was the nature of this change of attitude?

It is often stated that whereas the earlier economists had viewed economic life in terms of *what ought to be*, the economists of the eighteenth century began to view it in terms of *what is*. This is perhaps not too misleading a statement as a first approximation, but it is important to remember that in economics what ought to be and what is can never be separated as completely as the statement implies. If you are interested in what ought to be, you must necessarily be interested in what is, even if only to give you some sort of idea of what *can* be. I have always felt that the Canonists, for example, ultimately got their ideas about the just price not from Heaven but from earth—from a study of the prices which were in fact being fixed on competitive markets in their time. And there have never been many economists interested in the study of what is for its own sake purely: most of them have studied what is in order to help them more effectively to prescribe what ought to be. The classical economists, after all, were just as concerned to prescribe as the Canonists and Mercantilists had been: the only real difference here was that the classical economists' prescriptions tended to be passive rather than active.

I should prefer to describe the nature of the eighteenth-century change of attitude something like this: The earlier economists *began* with certain moral, political, and theological postulates, which they used as the basis for passing judgment on certain things which in fact happened in the real world, and for arriving at decisions about the proper economic policies to be pursued. These judgments on what happened, and these policy decisions, constituted the core of most economic writing prior to Quesnay and Smith—the Canonist doctrines of the just price and usury, the Mercantilist foreign trade policies, and so on. What Quesnay and Smith did, in effect, was to turn the old order of things upside down by proclaiming that the moral had to be derived from the physical rather than vice versa. They began with economic reality, investigated the laws and regularities inherent in it, and only then, at the end of the story, announced that these laws and regularities were ' just ' or ' moral '. And these laws were described as ' just ' not so much because they conformed to some pre-existing scheme of morality, or even because their operation apparently led to beneficent results, but rather because they were ' natural '—i.e., because they were necessarily inherent in the prevailing set of socio-economic relationships and manifested themselves objectively, automatically and independently of men's will.

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* Read to the Annual Conference of the British Society for the Philosophy of Science held at Oxford, September 1963.

One of the main causes of this startling change of attitude was the simple fact that by the second half of the eighteenth century the so-called ' rule of law ' in the economic sphere was becoming very much more apparent than it had previously been. People began to get interested in observing economic laws and regularities, in other words, because there were a lot more of them to be observed. The rapid development of market exchange and the price mechanism, and the economic unification they brought with them in the more advanced countries, were creating a type of economic system which could be usefully envisaged as a kind of gigantic machine. The movements and inter-relations of the working parts of this machine could be said to be ' subject to law ' in much the same way as the movements of falling bodies, say, were ' subject to law ', and many of the methods developed in connection with the natural sciences could be applied to the analysis of these movements and inter-relations. The challenge implicit in this new state of affairs—to discover the actual laws by which the economic life of men in society was governed—was accepted by certain brave spirits, and the science of economics was born.

This new science, as I have said, seemed to contemporary observers to have cut itself off more or less completely from value-judgments. And it was certainly true that economics had now blossomed out into a discipline in its own right, the core of which consisted in an apparently objective analysis of economic reality. In so far as value-judgments entered consciously and openly into this analysis at all, they did so not at the beginning, not as the starting point, but only at the end, as a sort of by-product of the analysis. And this by-product could be—and in fact soon was—dispensed with. Economists could now begin to concentrate on analysing the objective laws governing the shape and movement of the economic machine: their moral and political views about the way in which the machine worked, and their concrete policy proposals, could be regarded not as integral parts of the science of economics, but rather as excrescences upon it—necessary and important excrescences, perhaps, but excrescences nevertheless. Economic analysis as such could be regarded as objective, however much the uses to which economists (and others) put this analysis might be tainted by value-judgments.

This is, I think, roughly the view about the nature of their science which most present-day economists have been brought up to accept. And up to a point the view is a perfectly valid one. The main job of economists since the eighteenth century has indeed been to take the economic machine to pieces to see how it works; to find out what would happen if the size or position of one or other of its parts were altered; and generally, to learn enough about the manner of its working to decide whether it can safely be left alone, or whether it is likely to go wrong, and if so how to repair it. The motives of this study, and the practical policy conclusions one draws from it, may well involve value-judgments, but the study in itself does not *necessarily* do so. The core of traditional economic analysis, after all, consists of the simple laws governing the inter-relations of supply, demand and price; and it is very hard to detect any concealed value-judgments worthy of the name lurking beneath these laws, at any rate as they are usually formulated in the more reputable textbooks.

Nevertheless, we all know that in actual fact value-judgments *have* been imported into economic analysis, and to no negligible extent either. Why, exactly? One cannot adequately explain this by appealing to such facts as that all of us tend to see in reality what we want to see, or that every man has an urge to be his own politician, philosopher, and moralist. Mere human frailty and the do-it-yourself instinct cannot account for the *extent* of the intrusion of value-judgments into economics. The really basic point, I think, is simply that the machine which the economist studies is an incredibly complex one. Every part of it is dependent, in a very intricate way, upon every other part; it has a large number of different functions; and no part of it has the decency to stay still while you are examining it. The result is that *general* explanations of the way in which the machine as a whole works are likely more often than not to be of limited utility; and there can be no single, unique explanation of how any *part* of the machine works. Thus it becomes possible for alternative explanations to be put forward, and for ideological considerations to motivate the choice of one or other of them.

Let me give a simple concrete example. Take the phenomena of rent and profit in a society organised like ours. If we look at these incomes from a commonsense point of view, it is clear that their emergence and persistence, and their actual levels at any given time, depend upon the interaction of a large number of different factors. Among the most important of these, fairly obviously, are, first, the natural monopoly of the ownership of land and capital which is possessed by the social groups receiving rent and profit, and, second, the pricing process to which land and capital are subjected when they are bought and sold on the market. A *complete* explanation of rent and profit would require the inclusion of both these determining factors, to say nothing of a host of others. But an explanation which brought in all these factors could hardly be said to constitute a *theory*: the very function of scientific theorising, when it is faced with a situation in which everything depends upon almost everything else, is surely to seize upon some relatively independent element in this situation which can usefully be regarded as the main key to the understanding of the particular phenomenon being studied. Thus in the case of rent and profit, two alternative ways of looking at their determination are immediately opened up. On the one hand, you can start with the natural monopoly, base your theory primarily upon this, and then bring in the pricing process, if you want to, in your second or subsequent approximation. On the other hand, it is open to you to start with the pricing process, and bring the natural monopoly into the picture, if at all, only at some later stage.

Now neither of these ways of looking at rent and profit seems to me to be conspicuously more ' true ' or ' valid ' than the other. The criterion of choice between alternative theories of this sort *ought* to be simply their comparative usefulness in relation to the particular problem or problems upon which the analysis is required to throw light. But men being what they are, the criterion of choice which is in actual fact adopted is very often a moral or political one. There have been periods in the history of economic thought, for example, when people who were critical of the existing economic system were strongly attracted to some variant of the 'natural monopoly' theory of rent and profit, and condemned any kind of 'pricing process' theory as superficial. There have been times, too, when people who supported the system were attracted to some variant of the 'pricing process' theory, and condemned any kind of 'natural monopoly' theory as involving an illegitimate preoccupation with politics and ethics.

Given the extreme complexity of the subject-matter of economics, and given the wickedness of our world, it would be utopian to expect this intrusion of value-judgments into economic theory to disappear at the first puff of a rational wind. This is just another of those unpleasant things which one has to learn to live with. The trouble is that if we allow a rational wind to blow away the illusion of theoretical certainty with which our ideological biases provide us, we are left with nothing but the uncomfortable knowledge that many of our economic theories have only relative validity. Quesnay was not 'wrong' and Smith 'right' when they put forward apparently contradictory views about the productivity of manufacture: each view was in fact 'right' in relation to the particular economy in which it was put forward and to the basic problems of that economy. Marshall's and Keynes's theories of interest—to take a more controversial example—are not in stark opposition: the first may be a 'valid' explanation of interest in a Marshallian world, and the second of interest in a Keynesian world. Possibly knowledge of the essential relativity of economic theory is even harder to live with than the fact of ideological bias—we can always pretend, after all, that madness is something which only infects other people—and that is why we economists have not yet succeeded in turning our science into anything like a completely objective one.

Most of what I have said so far relates to economics considered as the study of the way in which the economic machine works. This is a fair description of the main content of economics—its analytical core, as distinct from its policy proposals—since the days of Quesnay and Smith. The motives of this study have varied greatly, of course. Quesnay and Smith, for example, wanted to show that capitalism promoted economic development more effectively than Mercantilist regulation. After the so-called 'marginal revolution' of the 1870's many economists wanted to show that capitalism allocated resources among competing ends more efficiently than any other system. In our own century, Keynes and Pigou wanted to show that capitalism would in fact do neither of these jobs very adequately without a fair degree of external interference. But for all these economists, the main aim was an objective, scientific study of what was and what tended to be in the real world. And in the case of all of them, value-judgments imported from outside the discipline affected, to a greater or lesser degree, the actual content of their analysis.

During the last ten or twenty years, however, there have been signs of the emergence of a new view of the basic function of economics—a view which, if it comes to predominate, may well remove more or less completely the particular source of ideological bias which I have just been discussing. In the theoretical sphere, this new view is mainly reflected in two very important recent developments. The first is the evolution of certain mathematical techniques, often subsumed under the generic title of ' operations analysis ', which are designed to assist in the deliberate solution by private and public managements of a number of difficult problems of economic choice. The second is the development of the branch of economics known as ' welfare economics ', a substantial part of which now consists in attempts to formulate a set of rules by which the various economic situations open to a society at any given time may be ordered and compared from the point of view of their relative desirability. Now the interesting thing about these new developments is that they are mainly designed to help us to make the ' right ' economic choices in situations in which the price mechanism cannot be expected to make them for us automatically. In other words, they do not presuppose the existence of what I have been calling the ' economic machine ': if anything, indeed, they presuppose the absence of the machine, at any rate in the particular segment of the economy being considered. And they do this, I think, for the simple reason that in the real world today the areas in which the machine does not operate are rapidly growing wider. In some of these areas the machine's writ cannot by the nature of things run. In other areas it has broken down, or has been deliberately destroyed. The result is that men are now becoming obliged to solve deliberately a number of important problems of economic choice which in former days were either relatively insignificant or which the machine automatically solved for them.

The breakdown and destruction of the machine is most obvious, of course, in the Communist countries. But in the West, too, owing to the increasing popularity of planning techniques, the emergence of large and complex public enterprises, and the increasing scale of individual private firms, the relative size of the area within which we can expect the price mechanism to make reliable economic choices for us is fairly steadily diminishing. The result is that economics, both in the East and the West, is being gradually transformed into a science—or perhaps an art—of economic management, or social engineering. The prophecies of Bukharin and Preobrazhensky, who in Russia in the 1920's foretold the impending doom of economics in its traditional form, are at last coming to be fulfilled. Just as in the eighteenth century, a radical change in socio-economic organisation is engendering a radical change in the nature and function of economics.

What role will value-judgments play in the new economics, then, if it does in fact continue to develop in this direction? About as great or as small a role, I suggest, as they play at the moment in the work of the engineer. The engineer is asked by his employer to produce a certain result as efficiently as possible, the criterion of 'engineering efficiency' being laid down in advance. Similarly, the economist of the future will be asked to allocate a given set of resources as efficiently as possible, to select the most efficient of a number of methods of doing a particular job, and so on, the appropriate criterion of '*economic* efficiency' being laid down in advance in each case. Under these conditions, and in relation to tasks of this type, the scope for the intrusion of value-judgments will be very small indeed.

Many economists, however, will almost certainly want to have a special say in the formulation of the appropriate criteria of economic efficiency, and here there will undoubtedly be plenty of scope for the intrusion of ideological bias. The recent attempts by economists to evolve welfare criteria which would be internal to the discipline, so to speak, have hardly been very successful. But once the necessary criteria are defined, of course—whether by the economist or for him—the economist's job will become very similar indeed to that of the engineer, and his conclusions will become testable in a way in which a theory of profit or rent, say, is not.

One does not quite know whether to be pleased or sorry that the days of the intrusion of value-judgments into 'positive' economics are numbered. On the one hand we economists would like our subject to be truly scientific; on the other hand it is largely the neuroses and ideological biases of our fellow-economists (never of ourselves, of course) which make the subject interesting. I can visualise a great division of labour developing among the economists, or social engineers, of the future: the neurotics and ideologists will concentrate on formulating welfare criteria, bringing in value-judgments by the score, while the

VALUE JUDGMENTS AND ECONOMICS *[1]

KURT KLAPPHOLZ

I AM not sure whether Professor Meek is right in claiming that there was a conscious separation between economics and ethics as early as the eighteenth century. My impression is that in England, at any rate, this did not happen until rather later, around the middle of the nineteenth century. But whether we trace this development back to the eighteenth century or merely to the middle of the nineteenth century, it remains a rather curious fact that even present-day 'orthodox' writers on the methodology of economics still find it necessary to expound, and insist upon, the principle of ethical neutrality. I must confess I find this rather puzzling, since, as Professor Meek points out, this is the view of their subject which most present-day economists—he and I included—in any case accept. In the circumstances one would have thought that Professor Stigler was right when he commented that 'it does not seem necessary to retread familiar ground to show that economics as a positive science is ethically neutral'.[2] Yet a no less distinguished economist, Professor Haberler, while reviewing a recent *Festschrift*[3] welcomed 'the spirited defence of the principle of *Wertfreiheit*' provided by one of the contributors and regarded it 'unfortunately nowadays more needed than ever despite Cairnes, Max Weber, Pigou, etc. . . .'

Haberler did not explain the urgency of this need; but he may have had in mind what Professor Hutchison calls the recent 'more massive wave of criticism or scepticism of *Wertfreiheit* from leading writers than perhaps at any time since Senior and J. S. Mill first drew the distinction [between normative and positive propositions] for economists'.[4]

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* Read to the Annual Conference of the British Society for the Philosophy of Science held at Oxford, September, 1963.

[1] I should like to thank L. P. Foldes and Alan Musgrave for their meticulous criticisms and helpful suggestions.

[2] G. Stigler, 'The Politics of Political Economists,' *Quarterly Journal of Economics*, 1959, **73**, 522

[3] G. Haberler, Review of: *Money, Growth and Methodology and other Essays in Economics*, ed. by Hugo Hegeland, *American Economic Review*, 1963, **53**, 145

[4] T. W. Hutchison, *'Positive' Economics and Policy Objectives*, London, 1964, p. 18

A succinct statement of this rather heretical position is given by Myrdal:

> There is no way of studying social reality other than from the viewpoint of human ideals. A 'disinterested social science' has never existed and, for logical reasons, cannot exist. The value connotations of our main concepts represents our interest in a matter, gives direction to our thoughts and significance to our inferences. It poses the questions without which there are no answers.
>
> The recognition that our very concepts are value-loaded implies that they cannot be defined except in terms of political valuations. It is, indeed, on account of scientific stringency that these valuations should be made explicit. They represent value premises for scientific analysis; contrary to widely held opinions, not only the practical conclusions from a scientific analysis, but this analysis itself depends necessarily on value premises.[1]

This statement suggests some of the preoccupations which allegedly give rise to the so-called value problem in the social sciences, and which I shall now examine.

Myrdal's view appears to contain an outright denial of the 'orthodox' position, formulated with beautiful simplicity by J. N. Keynes as long ago as 1890: 'the proposition that it is possible to study economic uniformities without passing ethical judgements or formulating economic precepts seems in fact so little to need proof, when the point at issue is clearly grasped, that it is difficult to say anything in support of it that shall go beyond mere truism'.[2]

I said that Myrdal's view appeared to contradict the 'orthodox' view. If taken literally, it certainly does, for according to the orthodox view economic hypotheses are compatible with any value premises and are therefore independent of them. Perhaps we should not take Myrdal literally. In any case, it may be useful to begin by recapitulating the 'orthodox' position.

The 'orthodox' position rests on Hume's observation that norms or proposals cannot be deduced from descriptive statements alone,[3] a descriptive statement being defined as a statement which has truth-value (whether or not it is possible to ascertain its truth). Thus, statements which have truth-value are value-free, in the sense that proposals cannot be deduced from these statements alone; *logically* they have no ethical implications.[1] Since the 'scientific part of economics consists exclusively of descriptive statements, it cannot have any ethical entailments, and is therefore value-free '.[2]

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[1] G. Myrdal, *Value in Social Theory*, London, 1958, pp. 1-2

[2] Quoted in T.W. Hutchison, op. cit. p. 36

[3] If this view is not accepted, and one advocates, for example, some kind of naturalism, then one must presumably regard value impregnation as universal, and not a special problem of the social sciences.

If this is what we mean by 'value-free' then, as J. N. Keynes said, it seems trivially true that economics, and the social sciences in general, can be value-free. To say that in talking about problems of the social sciences we cannot avoid making value judgments, or that what we say *necessarily* implies value judgments, suggests that it is the 'nature' of the subject that dictates how we talk about it.[3] But this is clearly not so; whether or not we choose to utter sentences that have half-truth value depends on us. In this respect the social sciences in no way differ from other fields of discourse.

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[1] I am aware that there is still controversy as to whether the distinction between 'is' and 'ought' is purely a matter of logic; see, for example, A. Montefiore, *A Modern Introduction to Moral Philosophy*, London, 1958, chapter 8. For my purposes it is sufficient to point out that the 'orthodox' position requires only the impossibility of deducing norms or proposals from factual statements. It is not clear whether the critics of *Wertfreiheit* go as far as to deny this impossibility.

[2] This is the sense in which most 'orthodox' writers have regarded economics as value-free, cf. for example, L. C. Robbins, *The Nature and Significance of Economic Science*, 2nd edition, London, 1948, pp. 147–151. However, advocates of *Wertfreiheit* occasionally formulate the principle in a much wider sense. Professor Haberler, for example, a staunch advocate of ethical neutrality, commended Pigou's view of *Wertfreiheit* thus: 'Since Pigou's excellent formulation, which is all the more impressive because it avoids economic and philosophical jargon, is not well known, let me quote it: "If the 'art' of social reform is to be effective, the basis of it must be laid in a 'science'. The contribution towards the work of practice that economists aspire to make is to provide for it this foundation. Their effort, though it may well be roused to action by the emotions, itself necessarily lies within the sphere of the intellect. Resentment at the evils investigated must be controlled, lest it militate against scientific exactitude in our study of their causes. Pity however sincere and grief however real are here intruders to be driven ruthlessly away. Stirred by their appeal we have entered the temple of science. Against them its doors are closed, and they must wait without for our return"', *Unemployment, New York*, Home University Library, 1913, pp. 10–11. G. Haberler, op. cit. p. 145.

Gerschenkron also seems to suggest that the principle of *Wertfreiheit* precludes '. . . letting the scholar's own value judgements invade and pervade his research', A. Gerschenkron, 'Reflections on Ideology as a Methodological and Historical Problem' in *Money, Growth and Methodology and other Essays in Economics*, ed. by Hugo Hegeland, Lund, 1961, p. 184, n. It may not be easy to decide whether any particular scientific effort is value-free in this sense, and whatever one may think of these recommendations for scientific conduct, they must be not confused with the logical principle of ethical neutrality.

[3] Part of the controversy regarding *Wertfreiheit* relates not so much to its possibility as to its practice by economists, and is thus an aspect of the accusations made against economists of (conscious or unconscious) bias. I am not competent to appraise the merits of these accusations. Robbins, for example, regarded them as unjustified

I am not sure whether those who insist that economics is inevitably 'value impregnated' would necessarily disagree with what I have just said: as Professor Hutchison notes, some of the criticisms '. . . do not seem necessarily irreconcilable with the "positivist" doctrines. . . .'[1] I have merely re-stated the 'orthodox' view regarding the impossibility of inferring proposals from hypotheses. Those who insist on inevitable 'value-impregnation' are often concerned to show the relevance of 'values' to economic research and policy discussion. This is true—indeed trivially true, since values are relevant to all human activities—but it in no way affects the 'orthodox' claim of logical ethical neutrality.

It would be tedious to rehearse all the arguments paraded in support of the thesis of 'value-impregnation'. I shall merely pick out some to illustrate my point that they leave the 'orthodox' position untouched. Before doing so I must make one reservation. The charges of 'value-impregnation' have been advanced in the course of academic discussion of the 'orthodox' position, but their target may well have been different. They may have been intended as—often justifiable—criticisms of the public pronouncements of economists. The public may well commit the naturalistic fallacy, and the academic economist, by the phrasing of his pronouncements, may well encourage this. In this sort of context honesty perhaps demands that we proceed in the manner described by Weber. He suggested that a writer who accepts 'the absolute heterogeneity' between description and prescription,

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(see op. cit. p. 151, n.) H. Albert, on the other hand, claimed in 'Das Werturteilsproblem im Lichte der logischen Analyse' (*Zeitschrift für die gesamte Staatswissenschaft*, 1956, 112, 437), 'dass die ökonomische Wissenschaft von mehr oder weniger versteckten Werturteilen wimmelte und dass ihren Vertretern die Einsicht in diesen Zustand und in seine Bedeutung oft fehlte. Sie meinten ein System von Wahrheiten konstruiert zu haben und boten teilweise ein System, in dem gewisse Direktiven eine dominierende Rolle spielen.'

I suspect that the accusations and denials of bias are one of the root causes of the *Wertfreiheit* debate. From a methodological point of view, however, such accusations and denials are quite uninteresting and irrelevant. Hutchison suggests (op. cit., p. 50) that the clear-cut distinction between normative and positive has the 'apparent implication' of excluding 'bias' from economic science. Perhaps this is sometimes the motive for stressing the distinction, as I shall argue below; but in fact it has no such implication.

[1] T. W. Hutchison, op. cit. p. 47

but chooses to discuss both in the same breath, should make sure that 'he does not unwittingly (or just to be clever) deceive his readers concerning the absolute heterogeneity of the problems. Personally I am of the opinion that nothing is too "pedantic" if it is useful for the avoidance of confusions'.[1] It was partly because the public might read ethical content into terms like 'national income' or 'community welfare', even though economists use them in a purely descriptive sense, that Mr Little regarded sentences containing such terms as emotive and therefore ethical.[2] As Mr Archibald pointed out: 'It seems that there is some confusion between two separate questions, the domain to which welfare economics belongs, and the language in which the results of economics are best communicated to the public. If we try to settle the former question by reference to the latter, we shall indeed get into a muddle.'[3] I am in no way concerned with the latter question, important though it may be.

Let me now briefly consider some of the arguments advanced in support of the thesis of 'value impregnation'.

(1) With the decline of belief in induction, or what Myrdal calls 'naive empiricism', comes the realisation that, in order to get answers, we must ask questions; and this leads to the shattering discovery that the questions we ask may be influenced by our values! Thus values are inextricably involved from the start. This immediately shows that economics is not ethically neutral, for now it is clear that our values partly determine our view of what is practically possible. No one, I think, would wish to deny the truth of this discovery. The only surprising thing is the almost exclusive concentration on 'values' as being the over-riding factor, as though imagination, ingenuity, and sheer technical skill were of negligible importance.[4]

In any case it is clear that this discovery, despite vague suggestions to the contrary, in no way affects the logical neutrality of economics.

'Neutrality' sometimes seems to be understood in a wider sense, namely in the sense that the descriptive statements one believes either need not, or perhaps cannot, influence one's ethical views. Perhaps Professor Smithies had this notion of neutrality in mind when he said that hardly 'any economic theory can be considered ideologically neutral'.[1] This again seems trivially true; Keynes's *General Theory*, I am glad to say, was not neutral in this sense. But this sort of 'neutrality' obviously has nothing to do with logical neutrality; and I do not know whether anyone ever asserted that economics or any other science was neutral in *this* particular sense.[2] This view of 'neutrality' may have been suggested by such statements as 'economics is neutral as between ends'[3]; or that 'economics as a positive science is ethically—*and therefore politically*—neutral'.[4] But it is clearly an untenable view, for it would imply that factual arguments have no place in moral discourse.

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[1] M. Weber, *The Methodology of the Social Sciences*, ed. by E. A. Shils and H. A. Finch, Illinois, 1949, p. 20

[2] I. M. D. Little, *A Critique of Welfare Economics*, 1st edition, Oxford, 1950, chapter 5

[3] G. C. Archibald, 'Welfare Economics, Ethics and Essentialism,' *Economica*, 1959, 26, 324

[4] 'Surely, the scholar's processes of selecting his subject and his approach may be, and probably are, a good deal more variegated . . . than would appear from describing them as products of value judgements', A. Gerschenkron, op. cit. p. 183.

The difficulties which arise from a confusion between ethical utterances and 'persuasive' utterances are well illustrated by Mr Little's discussion of the ethical content of economics. On the one hand, Mr Little apparently wanted to define value judgments as 'persuasive' statements; on the other hand, he was well aware that purely descriptive statements could be persuasive. There was, therefore, a danger of arriving at the conclusion that *all* statements could be persuasive, and *could* therefore be value judgments. In an attempt to avoid this dilemma, Mr Little suggested:

> we may add that it is not sufficient that a value judgement should influence people. One can influence people by pointing out the consequences of the moral or aesthetic attitudes which they have to certain facts. A value judgement is one which tends to influence them by altering these beliefs or attitudes.[5]

As far as I can see, this suggestion quite fails to clarify the distinction between persuasive utterances which supposedly are, and those which are not, value judgments. Of course, I do not think one need attempt to make such a distinction; one need merely remember the logical principle of ethical neutrality, but at the same time admit that factual statements in economics may be just as ' persuasive ' as value judgments.

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[1] A. Smithies, 'Economic Welfare and Policy' in *Economics and Public Policy*, Brookings Lectures, Washington, D.C., 1954, p. 2

[2] 'Our policy is determined in the light of the facts, but is not deduced from them'. J. A. Passmore, 'Can the Social Sciences be Value-Free?' in *Readings in the Philosophy of Science* (ed. H. Feig land M. Brodbeck), New York, 1953, p. 675

[3] L. C. Robbins, op. cit., p. 147, though I don't think Robbins held this view. The 'ends' as regards which Robbins considered economics 'neutral' were 'ultimate judgements of value'. Robbins did not explicitly discuss the status of 'ultimate judgements of value'; but we may interpret them as referring to the decision-maker's 'tastes' or 'preferences'.

[4] G. Stigler, op. cit. p. 522; italics added.

[5] I. M. D. Little, op. cit. p. 72

(2) Just as the selection of questions may be ideologically biased, so also, as Professor Meek pointed out, may be what we accept as a ' true ' answer to these questions. The hypothesis that what we regard as a ' true ' answer is often a function of ideology might well hold in the case of economists. Thus, gazing at the same statistics, left-wing economists tend to ' infer ' from them that progressive taxation does not have a disincentive effect, while right-wing economists tend to ' infer ' that it does. Again, left-wing economists have thought that international elasticities of supply and demand were too small for devaluation to correct a balance of payments deficit, in which case direct controls might have to be imposed, while right-wing economists have thought the elasticities were sufficiently large for devaluation to work. Right-wing economists have tended to favour the hypothesis that recent inflation has been caused by excess demand, while left-wing economists favoured the cost-push hypothesis.[1]

Having added to the examples given by Professor Meek I merely wish to say that I do not accept his view of the ' relativity of economic theory '. Two contradictory theories cannot both be true, unless, of course, they are only apparently contradictory. But do these examples, assuming them to be correct, refute the ' orthodox ' view of *Wertfreiheit*? Quite obviously they do not, since the motives for accepting a statement as true or false can in no way affect its logical status (which depends only on its having truth value, not on its being true).

What these examples do show, however, is that testing of theories, one of the hall-marks of scientific activity, is still in its infancy in economics. My impression is that economists often attempt to test theories which, as they stand, simply are not testable.[2] This, of course, does not mean that they are necessarily tautological, but rather that they are metaphysical. And while, from the point of view of the sociology of knowledge, it may be interesting to ask why particular people believe particular metaphysical theories, this question is clearly irrelevant to the logical fact-value problem.

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[1] For a discussion of such ideologically conditioned belief in hypotheses see F. A. Lutz, ' Politische Überzeugungen und nationalökonomische Theorie,' *Ordo*, 1957, 60.

[2] Professor M. Friedman, ' The Methodology of Positive Economics ' in *Essays in Positive Economics*, Chicago, 1953, argued we should test economic theories, especially those appertaining to competitive situations, by deducing from them predictions about prices and quantities. However, Professor K. J. Arrow remarked that the ' price-quantity implications of the competitive model . . . are not easy to derive without major—and, in many cases, impossible—econometric efforts '. K. J. Arrow, ' Uncertainty and the Economics of Medical Care, ' *The American Economic Review*, 1963, 53, 944, which suggests that he does not regard the theory as testable.

(3) I come now to the claim that the concepts of economics '... are normative... in the sense much discussed recently, that they may have emotive connotations and may be used to persuade or to commend, as well as to describe...'.[1] While Mr Streeten seems to think that this applies to all concepts, Mr Little reserved it mainly for certain concepts of welfare economics.

I have no doubt that economics—or any other branch of learning for that matter—can be used in a 'persuasive' manner. And I also do not doubt that economics has in fact often been used in this way. But I have already disclaimed any interest in 'persuasiveness', at least for my present purpose.

As Mr Streeten points out, the view that economics is 'value impregnated' because it employs 'emotive language' and 'persuasive definitions' has only become popular recently. However, the prevalence of emotive language was noticed long ago, for example by Cairnes, who criticised Bastiat for using 'passionate terms':

> Availing himself of the double meaning of such 'passionate' terms as 'principle', 'value', 'worth', 'service', and the like, he has produced a theory which affects to cover both solutions—at once to explain and to justify the facts to which it applies. The economic vocabulary unfortunately lends itself only too readily to this sort of theorizing, and few writers have entirely escaped illusion from this cause.[2]

Although Cairnes did not argue that the intrusion of such language by itself renders economics ethical, Mr Little does, at least with regard to welfare economics. G. C. Archibald has refuted Little's argument, at least to my satisfaction. The objection to Little's view may be summarised as follows: the notions of 'emotive language' and 'persuasive definitions' may be relevant and interesting from the point of view of psychology and sociology and also, perhaps, from the point of view of the 'sociology of knowledge' (an inquiry particularly prone to confuse and even identify the two utterly distinct questions: 'is the statement X true?' and 'what were the motives for making the statement X?'). But it is clear that these notions, which hinge on the motives for, and the psychological effects of, using certain words, are utterly irrelevant to the logical status of different propositions.

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[1] P. Streeten, 'Introduction' to G. Myrdal, op. cit. p. xx

[2] Quoted in T. W. Hutchison, op. cit. p. 34

(4) While the various arguments purporting to demonstrate 'value impregnation', which I have listed, have come mainly from 'unorthodox' sources, the charge that early welfare economics illegitimately imported 'value judgments' was made by no less an 'orthodox' writer than Professor Robbins. I want to digress for a moment to consider Robbins's argument, and the consequences for welfare economics of accepting it.

Early welfare economics made use of the notion of 'social utility' as the summation of individuals' utilities. Robbins held that this notion had '. . . no place in pure science',[1] because interpersonal comparisons of utility were untestable by observation, introspection, or by asking questions. Of course, such comparisons had to be made in everyday life; but they were 'arbitrary' and 'conventional', and hence normative. Therefore a welfare economics which incorporated such notions was also normative.

Robbins's view that interpersonal comparisons of utility are value judgments, and therefore have no place in 'scientific' welfare economics, seems to have been widely accepted among economists,[2] despite the fact that Robbins's argument rested on a confusion between untestable statements and value judgments, and hence involved a contradiction. Thus Robbins thought that, in connection with interpersonal comparisons, assumptions were made '. . . which, whether they are true or false, can never be verified . . .'[3]; but if interpersonal comparisons can be either true or false (even though we may have no way of knowing which is the case) then by definition they cannot be value judgments. On the basis of Robbins's argument we must conclude that, whether or not we regard interpersonal comparisons as 'scientific', we cannot regard them as value judgments.

Here I should like to deal with another point, also made by Archibald, which has a bearing on the question of interpersonal comparisons of utility. Archibald insisted that we should not engage in 'essentialist' arguments by asking whether or not it is permissible to christen this or that index a 'welfare index'. We could, if we wished, christen some index of the level of employment, or of income distribution, a 'welfare index'. The old question as to whether it is possible to measure welfare within 'scientific' welfare economics would then disappear by virtue of the choice of definition (or would at most become a technical problem of measuring the particular index selected).

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[1] L. C. Robbins, op. cit. p. 139

[2] Thus, G. C. Archibald wrote: '. . . Robbins' view [that welfare economics was normative] I believe to have been correct given that welfare economics depended upon (untestable) interpersonal comparisons of utility, but not otherwise. . . .' op. cit. p. 317, n. 1. This quotation brings out clearly the confusion between value judgments and untestable statements which recurs in writings on economic methodology. In K. J. Arrow, op. cit. p. 942, we also read that '. . . interpersonal comparisons, are of course, value judgements'.

[3] L. C. Robbins, op. cit. p. 137

Archibald was, of course, right about the futility of essentialist arguments. But the matter is not quite so simple. Welfare economics is, and has always been, individualistic; that is to say, it proceeded on the assumption that there was no such thing as an independent community interest apart from the interest, or the welfare, of the individuals comprising the community. In welfare economics. 'community welfare' has always been understood in this sense.[1]

Now if we say that an increase in employment is 'an increase in welfare', we must be aware that this increase in employment, while it benefits some people, may harm others. If we stick to our individualistic view of 'community welfare' we must therefore be making interpersonal comparisons of utility between those who benefit and those who are harmed. Thus the problem of interpersonal comparisons of utility cannot be resolved merely by defining a welfare index. If we follow Archibald, and simply *define* 'an increase in employment' as an increase in welfare, we ignore, but we do not solve, the problem of comparing the benefit of the change to some members of society with the loss to others. Now in welfare economics the focus of interest has been precisely on the question as to which policy measures promote the interests of the members of the community. If we want to discuss *this* question, we have to make interpersonal comparisons, except in the (presumably) exceedingly rare cases in which a proposed policy change commands unanimous approval. Archibald's method simply means that we do not discuss this question.

I trust that my criticism of Archibald is not tainted with essentialism, and pass on to consider one more argument for the view that economics is inevitably 'value impregnated'.

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[1] Archibald seems to have overlooked this consequence of individualism. Thus, he complained that 'It sometimes appears to be suggested that, because the criterion in welfare economics is the individual's utility function, the conclusions have, in some sense, a moral superiority to conclusions about other criteria such as employment or income distribution', G. C. Archibald, op. cit. p. 322.

The criterion of individual utility does not necessarily have any moral superiority, but it is the only criterion consistent with an individualistic view of social welfare.

(5) It has been part of the ' orthodox ' position that the ' economist *qua* economist ' cannot recommend the adoption of one policy in preference to another; that is to say, he cannot, *qua* economist, give imperative advice. This seems to me a rather inept way of restating the logical point that no prescriptions can be deduced from descriptive statements.

However, according to the ' orthodox ' view, the economist *qua* economist could give advice scientifically, if he restricted himself to hypothetical imperatives. It was in this context that the means-ends dichotomy was often employed. If the ' ends ' are given, say by a political authority, the economist can ' scientifically ' advise on the best means for their realisation.

Although the means-ends dichotomy is rather ancient, people repeatedly discover that it is not a clear-cut dichotomy after all. Thus Mr Streeten points out that ' People do not attach value only to ultimate ends (whatever this may mean); and they are not indifferent between the means which promote these ends, even where the means are technically and otherwise exactly equivalent '.[1]

Apparently the possibility of value-free, i.e. hypothetically imperative, advice rested on the possibility of a clear-cut separation between means and ends. The fact that this clear-cut separation often may be impossible ' . . . greatly restricts the sphere in which prognosis can progress without assuming or committing itself to specific valuations almost all the time. It narrows the realm of " neutrality " and " objectivity ", as defined by the means-ends protagonists.'[2]

Thus, the notion of ' value-free ', ' scientific ' advice, which may be unobjectionable where purely engineering problems are concerned, is apparently illusory when we come to problems of social policy.

Everyone will agree that economists—and many others such as defence experts—often give highly value-loaded advice. Nevertheless I do not think that the absence of a clear-cut dichotomy between means and ends renders ' value-free ' or ' scientific ' advice impossible; whether or not it is impossible seems to me to be purely a question of the possibility of making a valid inference.

Value-free advice requires that we be given ethical premises which, in conjunction with empirical hypotheses and initial conditions, enable us to deduce a prescription. If, and only if, a deduction is possible, the advice will be ' value-free '.

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[1] P. Streeten, op. cit. p. xxi

[2] P. Streeten, op. cit. p. xxii

To illustrate with a trivial example: suppose we have a prescriptive premiss such as:

(*a*) ' The government ought to take *any* measure to raise the level of employment whenever the level of unemployment exceeds x %.'
If we add

(*b*) ' The present level of unemployment exceeds x % '
and

(*c*) ' Measure A will raise the level of employment '
we can deduce

(*d*) ' The government ought to take measure A.'

Were an economist furnished with (*a*) and he could provide (*b*) and (*c*), his ' advice ' (*d*) would be ' value-free '. In this example, however, in which only one single, exclusive, aim of policy appears, there is really no problem of choice. Any policy measure which raises the level of employment is an ' appropriate ' measure and, given the single aim, as ' good ' as any other. Policy makers rarely, if ever pursue single aims: instead they pursue a number of ' conflicting ' aims, in the sense that the achievement of ' more ' of one aim can only be obtained at the cost of ' less ' of another.[1]

In these circumstances the logical derivation of the appropriate policy requires (*a*) a knowledge of the policy-maker's ' utility function '[2] and (*b*) a knowledge of the ' possibility function ' which describes ' how much ' of one aim has to be sacrificed for an increased attainment of another. Once again, if an economist were furnished with his ' client's ' utility function, which, together with the possibility function provided by the economist, made it possible to derive the ' appropriate ' policy, the resulting advice would be value-free. It may be noted that if we state the problem in this way, we need not bother with the slippery means-end dichotomy. Instead, we simply ask: given the various possible courses of action (as described by the possibility function) what is the ' best ' course, given the utility function? It should be clear, once again, that the absence of a clear-cut dichotomy between ' means ' and ' ends ' does not logically preclude the possibility of value-free advice.

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[1] Where the aims of policy are all complementary, i.e. where the achievement of ' more ' of one aim also yields ' more ' of another, there is again no problem of choice.

[2] ' When someone speaks of " given ends ", then, strictly speaking, he must mean a given utility function ', H. Giersch, *Allgemeine Wirtschaftspolitik*, Wiesbaden, 1960, p. 52. Translation provided.

I have tried to elucidate the logical conditions in which conditional advice may be said to be value-free. I shall consider in a moment whether an 'expert' economic adviser could ever be in a position to give value-free advice, in the sense defined. I have concentrated on the logical aspect of this question, because this aspect has been much discussed in the literature,[1] though not always with as much clarity as might be wished. In particular, there appears to have been a failure to stress the condition of deducibility. For example, Mr Streeten tried to formulate the conditions for value-free advice as follows:

> As we have seen [the means-ends dichotomy] was meant to circumscribe a neutral sphere in which objective statements can be made, and into which no valuations enter. But if values are attached to means, the qualifications required by the acceptance of the ends become necessary at every stage. If end E can be brought about by courses *a*, *b* and *c*, and if no direct values are attached to *a*, *b* and *c*, a scientific discussion of these courses, which abstracts entirely from valuations, is possible: how long they take, how effectively they promote E, what other results E_1, E_2, etc., besides E they are likely to have, etc. It is then possible to arrive at the conclusion, say, that *a* is the most effective course, *if E* is desired and if E_1, E_2, etc., are insufficient deterrents (or welcome byproducts). This is the model commonly envisaged by practitioners of economics.
>
> But this procedure is no longer open if direct values are attached to *a*, *b* and *c* themselves. For then we can only say, tautologically, choose *a if* you want *a*, choose *b if* you want *b*, etc.: whereas before we could say, choose *a*, but not *b* or *c*, if you want E. Since the hypothetical clause referring to valuation has to be introduced at every stage, empirical analysis disappears altogether.
>
> The following quite unwarranted assumptions would be required if we were to discuss objectively means in relation to 'given' ends:
>
> 1. People attach to means no direct value, but only instrumental value.
>
> 2. People attach to ends direct value only, and never consider them as means to other ends.
>
> 3. No other effects of means than the 'given' ends have direct value.[2]

It would appear that, according to Mr Streeten, it is possible 'to discuss objectively means in relation to "given" ends' only when we have a prescriptive premiss of the following kind:

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[1] For a review of some of the discussion see T. W. Hutchison, op. cit. pp. 108–116.

[2] P. Streeten, op. cit. pp. xxi–xxii

> Whenever the level of unemployment exceeds $x\%$ the government ought to take any measure to restore the level of employment which (*a*) does so more quickly or no less quickly than any other, and (*b*) does not lead to a balance of payments deficit nor (*c*) to an inflation of more than 2% per annum.

If there were, for example, two measures which restored the level of employment with equal speed and satisfied conditions (*b*) and (*c*) they would be equally 'appropriate'. But if there were no single measure which satisfied all three conditions, no recommendation whatever could be deduced. Thus, according to Mr Streeten, we can 'discuss objectively means in relation to given ends' only when there is no 'conflict' among policy aims,[1] whereas economists have typically considered situations where such 'conflict' is present.

I have already pointed out that the fact that a policy maker may attach value to 'means' does not preclude the possibility of value-free, empirical advice. Mr Streeten's conclusion that, in this case, the advice can only be 'tautological', seems to depend on the assumption that people want (*a*) or (*b*) regardless of cost, i.e. that there is no problem of choice.

So much for the logical aspects of the problem of value-free advice. But can an economic adviser reasonably expect to be furnished with a set of 'policy ends' which would make possible the deduction of value-free advice? The answer, obviously, is 'No'. 'Ends of policy' are not simply 'given', but are elicited and modified in the course of discussion. This sort of discussion is bound to be 'persuasive' since its purpose is to persuade someone to adopt certain policy aims. Purely critical discussion, i.e. the pointing out of inconsistencies in policy aims, or of inconsistencies between aims and proposed policies, is inadequate, since by itself it cannot furnish a set of consistent aims. Some might perhaps urge that the expert adviser should confine himself to the critical parts of such a discussion; but it is doubtful whether such a restriction would be useful or feasible. We may thus easily agree with a former economic adviser to the U.K. government when he explained that 'it is an illusion to imagine that the dispassionate economist beloved of those who labour the distinction between ends and means, between normative and positive economics, can live in the practical world '.[1] Indeed, it is puzzling that anyone should ever have entertained this illusion.

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[1] 'If we believe that unemployment spells misery and loss of human dignity, and that it violates our belief in the brotherhood of men, but that some of it may at times be necessary to prevent greater evil, no simple criterion could be formulated, which would show whether, by avoiding unemployment, we have done the right thing.' P. Streeten, op. cit. p. xxiv.

In the context of this discussion, we could only say whether the 'right' thing was done if we were given the policy maker's utility function; given his utility function, it may be possible to determine *his* best policy. But, of course, it is impossible to determine a policy which is best in some absolute sense, i.e. irrespective of the policy maker's utility. It may be that Streeten had this point in mind, in which case he is right.

Nothing said in the preceding paragraph touches on the problem of *Wertfreiheit*, or on the problem of value-free advice. The latter, I have tried to argue, is a matter of deducibility; how the requisite value premisses come to be formulated—if they can be formulated at all—is altogether irrelevant to it.

I have tried to show that the various claims, often advanced '... with an air of penetrating profundity ',[2] that economics is *necessarily* value-impregnated can easily be refuted. Why, then, should criticisms which so clearly miss the target continue to be urged so vociferously? And why should the defenders of the ' orthodox ' position respond to these criticisms by reiterating the principle of ethical neutrality—rather as if present-day geographers continued to insist that the earth is not flat?

Any attempt to answer these questions must be speculative and interpretative. If the ostensible cause of a dispute seems spurious to the outsider, he can only try to guess at the real causes. I should like to conclude by suggesting some possible reasons for the persistence of this debate. These reasons are not meant to be exhaustive, nor is the order in which they are discussed meant to indicate their relative importance:

(*a*) Many still hold the view that any body of doctrine which can legitimately claim to be ' scientific ' is *ipso facto* respectable and authoritative, while the lack of this label robs a doctrine of respectability and authority. Since the conclusions of economics[3] appear unpalatable to all sorts of people, there is therefore an incentive to try to deprive economics of its supposed scientific status by claiming that it is 'value-loaded'. Similarly, those who insist on *Wertfreiheit* often seem to hold implicitly that the authority of economics depends crucially on this point.[1]

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[1] R. L. Hall, ' Reflections on the Practical Application of Economics, ' *Economic Journal*, 1959, **69**, 640

[2] T. W. Hutchison, op. cit. p. 50

[3] Recently there has been much more scepticism regarding the substantive conclusions of economics. As Stigler notes: 'The apparatus of economics is very flexible: without breaking the rules of the profession . . . a sufficiently clever person can reach *any* conclusion he wishes on any *real* problem. . . . In general there is no position . . . which cannot be reached by a competent use of respectable economic theory.' G. Stigler, op. cit. p. 531.

(*b*) Just as *Wertfreiheit* may be held to confer authority, so it may be held to guarantee the absence of bias, whereas 'value impregnation' may be held to imply bias. If it is thought that descriptive statements are 'unbiased' and the sole source of 'bias' is to be found in value judgments, then the absence of 'bias' is demonstrated by showing that economics is value free.[2] As I have suggested, the problem of 'bias' is one of the root-causes of the *Wertfreiheit* debate.

(*c*) The two sources of the debate concerning the problem of *Wertfreiheit* which I have considered so far appear to owe their influence to the fact that they have not been introduced explicitly into the debate, but appear nevertheless to have affected the motives of some of the participants on both sides. Once stated explicitly, their irrelevance is clearly apparent. The point to which I now turn has more substance.

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[1] Marshall, for example, held that although '... an economist, like any other citizen, may give his own judgement as to the best solution of various practical problems ... in such cases the counsel bears only the authority of the individual who gives it: he does not speak with the voice of his science'. Quoted in T. W. Hutchison, op. cit. p. 37.

The explicit reason often given for '... delimiting the neutral area of science from the more disputable area of moral ... philosophy' (L. C. Robbins, op. cit. p. 151) is that within the former differences of views can '... in principle be eliminated by the progress of positive economics' while as regards 'differences in basic values ... men can ultimately only fight'. M. Friedman, op. cit. p. 5. Yet in practice it is by no means clear that differences within positive economics are always more amenable to the solvent of rational argument than differences about values.

[2] This view appears, in fact, to have been sometimes adopted. Thus Robbins, for example, seems to have held that bias sprang from a 'confusion of the two kinds of proposition' (description vs. prescription). But he argued that economists of the 'orthodox' tradition had generally avoided such confusion and that 'most of the allegations of bias spring from an unwillingness to believe the facts that economic analysis brings to light'. These 'facts' could be deduced from theoretical economics as 'perfectly neutral inference(s)'. L. C. Robbins, op. cit. p. 151, n. Here ethical 'neutrality' seems to be identified with 'absence of bias'. Similarly, J. N. Keynes held that 'the attempt to fuse together enquiries as to what is, and enquiries as to what ought to be, is likely to stand in the way of our giving ... unbiased answers to any set of questions'. (Quoted in T. W. Hutchison, op. cit. p. 37.) Hutchison also suggests in one place that 'some of the arguments for the "impossibility" of maintaining normative-positive distinctions may be expressive ... of an unwillingness to face the costs of foregoing persuasion and influence, which a strict commitment to the distinction requires'. T. W. Hutchison, op. cit. p. 192. But on p. 194 he indicates clearly that one can be 'persuasive' and still maintain the distinction.

The urge to deprive economics of scientific respectability was perhaps stimulated by the claim—which, as the quotation from Stigler shows is no longer fashionable—that 'economic laws describe inevitable implications'.[1] An example of such a 'law' was 'the proposition that real wages above the equilibrium point involve unemployment'.[2] This prediction, however, like all scientific predictions, is subject to the fulfilment of certain conditions, e.g. regarding the state of competition in the particular market. A change in these conditions may mean that this prediction no longer follows from the theory. For example, the unemployment which may result in a free market from real wages being too high could perhaps be prevented by direction of labour. In general, conditions which enable us to deduce predictions from economic theories are themselves amenable to alteration by human action, so that we may be able to avoid certain consequences by a suitable change in those conditions. For this reason we can rarely say that a particular aim is unachievable.[3] These considerations have at times been insufficiently stressed in the traditional formulation of economic theory.

This suggests a possible reinterpretation of the doctrine of 'value impregnation' which may put its strictures on traditional economic thought in a more favourable light. This interpretation is simply that economics has been influenced by 'values' and 'ideology' in the sense that important questions which might have been asked were not, in fact, considered.[4] This failure is a legitimate, and possibly fruitful,

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[1] L. C. Robbins, op. cit. p. 121

[2] L. C. Robbins, op. cit. p. 151, n. Even though the claim of 'inevitable implications' is no longer fashionable, traces of it still persist, as, for example, in the view that a properly trained economist '. . . cannot believe that a change in the *form* of social organisation will eliminate basic economic problems', G. Stigler, op. cit. p. 528, a view echoed in practically every textbook of economics. Properly interpreted, this view states the hypothesis that scarcity cannot be eliminated by a change in social organisation. But contrary to the suggestion in the above formulation of this hypothesis, there are many 'basic economic problems' which can be affected by the form of social organisation.

[3] 'In discussing economic policies, some economists have described an objective—say socialism, a free market, or full employment—as "impossible", when all that they can be justified in saying is that on their scale of preferences the objective is not worth their estimate of the costs', T.W. Hutchison, op. cit. p. 192.

[4] A point illustrated with examples in G. Stigler, op. cit. pp. 530–532.

THE A PRIORI AND THE EMPIRICAL IN ECONOMICS *

RICHARD STONE

> The keynote of my *Plea* is that *the* work of the economist is ' to disentangle the interwoven effects of complex causes '; and that for this, general reasoning is essential, but a wide and thorough study of facts is equally essential, and that a combination of the two sides of the work is *alone* economics *proper*. Economic theory is, in my opinion, as mischievous an imposter when it claims to be economics *proper* as is mere crude unanalysed history. Six of y^e one, ½ dozen of y^e other!
>
> Alfred Marshall in a letter
> to F. Y. Edgeworth,
> 28 August 1902

1 *Introduction*

IN my talk this morning I shall describe the interplay of the *a priori* and the empirical in one field of economic analysis: the analysis of consumers' behaviour. What I am going to say illustrates an attitude to the work of the economist which was given expression by Marshall two generations ago in the sentences I have quoted from him. I hope I shall be able to convince you that from a practical point of view it does not matter in the least whether we begin with ' general reasoning ' or with a ' study of facts '; all that matters is that we should pass continually from one to the other so as to widen the range of phenomena to which we can give a coherent explanation.

Let me begin with a few general remarks. In studying the real world we isolate systems, and apply two methods of investigation. The first consists of making assumptions which can be combined into theories and given a particular form in terms of a theoretical model. The second consists of observing or measuring the components or features of the system so as to obtain a descriptive account of it and can be given the form of a practical specification. The combination of the two methods leads to a model. The purposes of constructing models are: (i) to understand how the system works; (ii) to calculate its outcome in different circumstances; and (iii) to indicate how it should be redesigned so as to function more in accordance with our wishes.

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* Read to the Annual Conference of the British Society for the Philosophy of Science held at Oxford, September 1963. A technical version of this paper appeared in *L'Industria*, 1963, No. 4, 467–86.

In setting the stage in this way I want to emphasise two features of scientific activity which I think are usually present. First, we are studying systems, that is things with interdependent parts. Second, we are interested in the real world and our models must not only satisfy conditions of consistency but also of realism; otherwise we shall only discover how a system of the kind we are studying might operate, not how the actual one we are studying does operate. I should add at this point that we do not begin by building, or even trying to build, perfect models. We begin by modelling the main features of the system we are studying and then try to improve on our prototype. Generally speaking we can never continue this process to its end; the world is so complicated that we must usually content ourselves with stochastic models.

If we examine what we do when we build models, it seems to me that the hard and fast distinction between the *a priori* and the empirical disappears. We build our theoretical models on a slender base of observation and then test them. Similarly our observations and measurements are based to some extent on concepts of what it would be illuminating to observe or measure. This sounds like a hen and egg proposition but that does not matter, because learning is essentially an iterative process.

2 *Demand Theory and Consumers' Behaviour*

The great economist Jevons, writing in the 1870's, laid it down explicitly that the theory of economics must begin with a correct theory of consumption and regarded it as unaccountable and quite paradoxical that English economists should, with a few exceptions, have ignored this doctrine. He rightly considered his theory of utility as a theory of consumption, and his ideas were further developed by his contemporaries and successors until in the works of Pareto we find a fully elaborated theory of consumers' behaviour which makes use of the idea of indifference surfaces. This theory was examined by Wold, some twenty years ago, in a series of papers in which he also demonstrated that other theories that had been propounded were equivalent to it provided that they were rendered internally consistent by including the integrability condition among their premises.

What do these theories amount to? They are concerned with an individual consumer who is assumed to have a certain amount of money to spend, usually called his income, and to be faced with a fixed set of prices. The satisfaction he gets from what he buys depends solely on the quantities of the different goods and services bought. His action is dictated by choosing that collection of goods and services which maximises his satisfaction, by the solution, that is, of a constrained maximum problem.

In order to apply this theory it is not necessary to measure satisfaction, or utility, indeed they need not even be measurable. All that is necessary is to work out the implications of the theory which relate to measurable phenomena and check the theory in this way. This amounts to little more than saying that the quantity of each kind of good or service that an individual consumer buys depends on his income and on the structure of prices.

Are we then free to choose any plausible form of relationship relating the quantities bought with income and the price structure? Not quite, because the theory places some restrictions on the relationships that are strictly compatible with it. In the first place, total expenditure must add up to income. In the second place, there must be no money illusion: the relationships must be homogeneous of degree zero in income and prices. In the third place, the Slutsky condition must be satisfied: the matrix of elasticities of substitution must be symmetric. These considerations apply if we do not assume a form for the utility function, that is the relationship between satisfaction or utility and the quantities of goods and services bought. If we assumed a manageable form for this function we should have determined the form of the demand relationships and these would then turn out to satisfy the restrictions I have mentioned.

I have set up this discussion of demand analysis in terms of a theory in search of observations. The theory relates to an individual consumer. But it is hard to obtain a large number of observations about an individual consumer and even if one could it is doubtful if one would accept the theory as a basis for empirical analysis. According to the theory, the individual's preferences are fixed and so he must not get married or have children, or they will surely change; probably he must not even age to any appreciable extent. His satisfaction depends entirely on his present purchases; it does not matter whether he already has a car or a wireless set. But of course all these things do matter and can be relied on to swamp the influence of income and prices in individual cases.

So before trying to apply the theory, we had better go a stage further and add up the individual demand functions for a whole community of consumers. This will have two effects. First, the particular circumstances of individual consumers will be pooled, and so will largely lose their significance. Second, the income of each individual will appear in the community, or market, function and this will involve an impossible and largely irrelevant number of variables. This difficulty is met by replacing the individual incomes by a single average income, to which may be added, if the information is available, the dispersion of the income distribution. In this way we end up with a model of consumers' behaviour in which the consumption per head of each commodity is a function of prices and one or more moments of the income distribution.

We can now apply this theory to observations. One way in which this can be done is to compile time series of consumption per head of different commodities, prices, and average income. The method employed is usually some form of multivariate regression analysis. In carrying out this work it is necessary to modify the model still further by grouping prices. Usually a small number of prices, relating to the commodity we are studying and to a few close substitutes or complements, are included separately and all other prices are combined into a single index-number.

3 *Lessons of Experience*

A considerable amount of experience with this kind of model applied to individual commodities is already available. The main lessons to be drawn from it can be summarised as follows.

(*a*) When applied to a fairly short period of time, such as the inter-war years 1920 to 1938, the model gives an illuminating first approximation to consumers' behaviour.

(*b*) It is usually necessary to recognise that consumers' preferences change over time. An allowance for this is often made by introducing a residual trend into the model.

(*c*) The model tends to work better for perishable goods than for durable goods. The reason is that the model is static, so that consumers are always in equilibrium. If circumstances change they will not as a rule be able to adjust immediately to the desired consumption of durable goods because these are usually expensive and pose a problem of finance that cannot be overcome immediately even with the help of hire-purchase. The model can, however, be adapted to meet this difficulty.

(*d*) In the case of new commodities, the community undergoes a learning process which cannot be represented by a simple, linear or exponential, trend in preferences: a sigmoid trend is necessary. Early writers on this subject, for example Roos and von Szeliski, used a symmetrical trend, in fact the logistic. More recently Bain, in his study of television ownership in Britain, has shown that a positively skewed trend is to be preferred. He in fact adopts a log-normal integral trend because of its convenient properties.

(*e*) If we look only at time series we ignore an important source of information, namely the cross-section data obtained from surveys of family budgets, which might require us to revise our estimates based on time series alone.

(*f*) If we consider demand equations in isolation, we run into the difficulty of identification due to the fact that prices and quantities are also related by supply equations. This is a big subject which I shall not develop here.

Thus we see that in trying to apply the classical theory of consumers' behaviour, we are led to a number of major modifications. The important ones relate to changing preferences, including the learning process, and to dynamic considerations connected, in particular, with durable goods. An anti-theorist might reasonably say that the relevant content of the theory was so small and so obvious and the factors omitted by it so important that we could dispense with the theory altogether. Very well, then: let us follow the anti-theorist and start from an empirical standpoint; not, perhaps, from observations in search of a theory but, as we shall see, from observations that may find it hard to get on without one.

4 *An Empirical Starting Point*

Let us suppose that we have time series of all the categories of goods and services bought by consumers, of the prices of these categories and of the number of consumers. We therefore also have time series of separate expenditures per head and of total expenditure per head. Let us now suppose, as a first approximation, that the individual expenditures are linear homogeneous functions of total expenditure and of each price. We may express this assumption by writing, in matrix notation,

$$\hat{p}e = b\mu + Bp \qquad (1)$$

where p denotes a vector of prices and ^ de
formed from this vector; e denotes a vector of
$\hat{p}e$ denotes a vector of expenditure; μ denote
b and B denote respectively a vector and a ma

The model given by (1), although simple, in
parameters. Thus if consumption is divide
groups, there are one hundred and ten paramete
is a formidable number in terms of the length o
tion actually available. So we must try to si
this purpose, the natural thing to do is to turn
above, we should like our demand equatio
condition that $\hat{p}e = \mu$ in all circumstances is
$i'B = [0, 0, \ldots, 0]$, where i denotes the u
denotes transposition. We also saw that we
functions to be homogeneous of degree zero in
demand functions in (1) already satisfy this c
no further restrictions. The final limitation
functions that we considered was the Slutsky
strictly applicable to market demand functio
abnormal circumstances we might expect it
If we assume that it does hold and also tha
independent of p, then, B must take the form

$$B = (I - bi')\hat{c}$$

where I denotes the unit matrix and $\hat{c}$ denotes a
from a vector of constants, c.

If we substitute for B from (2) into (1), we

$$\begin{aligned} \hat{p}e &= b\mu + (I - bi') \\ &= \hat{p}c + b(\mu - p' \end{aligned}$$

In (3) $i'b = 1$ and the elements of c are unre
commodities there are nineteen independen
mated, a great reduction over the 110 with wh
manageable number.

I shall now give a simple statement of th
implicit in (3). The average consumer has a
standard of living expressed by the elemen
goods at a cost of $p'c$ and so has an amount of
left over. He spreads this money over the
proportion to the elements of b.

We may all agree that this is a plausible, approximate model of consumers' behaviour in any particular period. We could hardly suppose, however, that the elements of c and, to a lesser extent, those of b would remain constant over time. It is impossible to add time trends on to (3) without destroying the theoretical properties of the model, but it is possible to make b and c functions of time or any other predetermined variable. Thus writing b_θ and c_θ for b and c at time θ, we could put

$$b_\theta = b^\star + \theta b^{\star\star} \tag{4}$$

and

$$c_\theta = c^\star + \theta c^{\star\star} \tag{5}$$

When applied to eight broad groups of consumption in Britain over the period 1900 to 1960, the improved model given by (3), (4), and (5) gives fairly good results. In the case of food and clothing these results are in conformity with estimates of total expenditure elasticities derived independently from budget studies.

We have seen how this particular model can be modified to allow for changes in preferences. It can also be modified to allow for adaptive behaviour. It has in fact an explicit form of utility function which implies ordinal utility and makes possible the construction of a constant-utility index of the cost of living. The whole development could therefore have been given a purely theoretical starting point.

We thus see the development of a model as an iteration between the *a priori* and the empirical. The danger in insisting too much on theories is that they are usually restricted and deal with the phenomena studied on limited assumptions, while at the same time theorists tend to insist on very general formulations within these limitations, formulations which are very hard to manage in practice. The model I have described contains such limitations: it cannot handle complementary or inferior goods; its Marshallian demand curves are never elastic. This would be enough for most theorists; throw it out, they would say. On the other hand it takes explicit account of changes in preferences and can be modified to represent adaptive behaviour, advantages besides which its limitations within the restricted field of pure demand theory seem, at least to me, rather unimportant.

5 *Conclusion*

I have only one conclusion to draw from this short survey of a single economic problem: the *a priori* and the empirical are completely intertwined in the development of the subject. True, there are specialists whose interests are mainly empirical or mainly theoretical. The first work with a rather small input of theory, the second with a rather small input of facts. But this is not the normal, healthy case; indeed, modern developments in economics have come about largely through breaking down barriers between excessive specialisms. I do not think that most of us could separate the contributions of the *a priori* and the empirical in the development of our ideas nor do I think that most of us would wish to deny the essential part played by each.

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EMPIRICAL AND A PRIORI IN ECONOMICS * †

ANNE MARTIN

> Nothing but unreality can come of political economy till we know when and how far its first assertions are true in matter of fact, and when and how far they are not.[1]

ECONOMISTS have often been regrettably unclear as to whether their 'first assertions' are *intended* to be true in matter of fact. Oskar Lange clearly thinks they are, calling the basic postulates of economic theory 'approximative generalisations of empirical observations'.[2] It is not clear, however, whether he thinks that the postulates are supposed to be directly testable, or whether they are selected because they are likely to lead to other testable hypotheses. Some equally eminent economists appear to have thought that they are chosen for their capacity to spawn logical consequences which together form an intricate and aesthetically satisfying pattern, regardless of whether or not any empirical test can be applied to them and of whether such a test usually strengthens or weakens them. For example, Mrs Joan Robinson once wrote, 'unless economics is content to remain for ever in the age of alchemy, it must resolutely turn its back on the pursuit of gold, however precious it may be to the human welfare, and embark upon the path of an austere and disinterested search, not for " the truth " but for a single self-consistent system of ideas'.[3]

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* The four papers making up this Number were the four papers on economics given at last year's Conference, and the four authors were asked for their contributions for publication. Miss Martin, not realising the possibility of publication, quite properly utilised material from a previous paper and article-review of hers in this *Journal*, even to the extent of making considerable quotation from them. She would, therefore, have preferred not to publish her paper.

It is unquestionably the policy of this *Journal* not to republish. None the less the Editor considers it justifiable to do so in this instance, and takes responsibility for it, because Miss Martin has put her excerpts together in a sharp and new way and because her paper is appropriate in the context of Professor Stone's.—Ed.

† Read to the Annual Conference of the British Society for the Philosophy of Science held at Oxford, September 1963.

[1] W. Bagehot, *Economic Studies*, London, 1908, p. 94

[2] O. Lange, 'The Scope and Method of Economics,' *Review of Economic Studies*, 1945, 13, 21

[3] Joan Robinson, *Economics is a serious subject*, Cambridge, 1932, p. 4

There are four particular errors to which economists have been particularly prone and which, between them, mostly account for the inability to agree about the status of their Postulates, or Laws.

The first error consists of treating an analytical proposition as if it were synthetic. As an example, let us take the proposition that a 'rational' entrepreneur will always try to equate marginal cost and marginal revenue. This is true in economic theory by virtue of the definition of 'rational entrepreneur' as one who tries to maximise monetary gains in the present or near future. As Lange puts it, 'a unit of economic decision is said to act rationally when its objective is the maximisation of a magnitude. Firms thus act rationally, by definition'.[1] It was Joan Robinson who pointed out that economists are always in danger of giving their purely formal statements a misleadingly empirical appearance, just because they have adopted a technical vocabulary to only a limited extent. Their theoretical constructs bear the name 'firm', 'entrepreneur', 'consumer', 'market', so that it is only too easy to understand a proposition where the word 'entrepreneur' should appear in inverted commas, as being an empirical statement about the actual behaviour of real-life entrepreneurs. And this has constantly been done, although by no means by all economists. Bagehot, for instance, wrote quite uncompromisingly 'English Economists are not speaking of real men, but of men as it is convenient to us to suppose that they are'.[2] This particular error is an insidious one because it gives the false impression that empirical research, which may badly need to be done, *has* in fact been done. How an entrepreneur behaves in adjusting his costs to his revenue is just the sort of thing one would expect to be able to discover by careful observation and enquiry.

The second type of muddle into which economists commonly land themselves and others is to start with a plainly synthetic proposition, find it inconveniently often falsified, and restate it so as to make it, in effect, analytic. The laws of Supply and Demand, as usually stated, provide an example of this, e.g. 'a rise in price tends, sooner or later, to decrease demand and to increase supply.'[3] In fact, of course, as in Britain today, we often find that a rise in price of a particular commodity is followed by an *increased* demand for it; or, as in the nineteen-thirties, that a fall in price is followed by a *decreased* demand. Rather than say that their account is often false, however, economists have adopted the procedure either of saying 'a rise in price *tends*, etc.', or of introducing the phrase *ceteris paribus*. Either procedure makes the proposition unfalsifiable, since the law can be exculpated either by saying that opposite tendencies must have been at work, or that other things didn't remain the same. For the 'law' to be testable, one has to define rather clearly what are the 'other things' that must remain constant. In physics, for example, Boyle's Law is not universally true and is understood as, in fact, applying only under certain conditions, e.g. of temperature, *which are carefully stated*. It has been argued, however, especially by Professor Hutchison,[1] that the '*ceteris paribus*' clause in economics usually performs an obscurantist function, since the factors to be included in the 'other things' are not specified, so that the supposition of any change whatever in the economic world saves the law. This criticism, however, may be unduly strong. Economists do, for example, specify with regard to the law of demand that consumer's tastes, incomes, and the availability and relative price of substitute commodities must remain the same. If a rise in price in present-day Britain is followed by increased demand, it is because incomes are also rising; while inverse reasoning applies to the nineteen-thirties. I think the real usefulness of these laws is found in their directive capacity; they say 'if demand, or supply, change, first of all look for a change in the price of the commodity for an explanation; if that does not do, try the other factors'. Similarly, with the 'law' of entrepreneurs maximising their profits. This is best understood as directing us to observe whether or not entrepreneurs are in fact equating marginal costs with marginal revenue and, if not, to start looking for the other reasons, which may be caution (i.e. regard for maximum profits in the longer run), preference for leisure, or ignorance.

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[1] O. Lange, op. cit. p. 30

[2] W. Bagehot, op. cit. p. 7

[3] H. D. Henderson, *Supply and Demand*, Cambridge, 1932, p. 19

The third type of confusion to which economists have been prone is a failure to distinguish between rules which are essentially guides to the construction of economic *theory* and rules which are guides for economic *policy;* between rules which help us to describe reality, and those which help us to criticise reality. It is hard to believe that this confusion has not at times been wilful. It is sometimes exemplified in an ambiguous use of the word 'ideal'. The state of perfect competition, for example, is often thus qualified and it is not always clear whether we should understand this as meaning (1) a simplified model, like a frictionless world in mechanics, which is convenient as a starting-point for explanation, or (2) perfect, in some normative sense. We may agree that economic laws are ' directives for the investigator to find his way about in reality '[1] or heuristic postulates,[2] but it does not follow that they should be taken as imperatives of economic *behaviour*. This later view has, however, been held by distinguished economists. Professor F. H. Knight, for instance, writes, ' it is the function of economic principles to describe the ideal, not the reality; and it is an ideal for social policy and not merely the pattern to which the individual wishes and tries to conform '.[3] Professor Ludwig von Mises puts this viewpoint more extremely when he writes, ' knowledge of Political Economy leads necessarily to liberalism '.[4] Whatever we may think of the implications of this view for economic policy, it can only have a harmful effect in the method of economic study. If we treat economic laws as imperatives in *this* sense, we lose any possibility of testing the propositions we derive from them. As long as a study results in no propositions which can, at least in principle, be subjected to empirical test and thereby falsified or strengthened, it can only be pseudo-science. This is what Professor Knight seems to wish to make it when he writes, ' it is not conceivably possible to verify any proposition about economic theory by any empirical procedure. Economy involves an intention or intended result which is not amenable to observation in any admissible use of that term.'[5] If the propositions about economic theory were really about intentions, it might perhaps seem that introspection, or *verstehende*, would be one method of testing them; although it would be open to doubt whether it was the best way, and I shall touch on this later. The task of economics, however, is to explain the economic behaviour of *aggregates* of individuals, and its results and, for this purpose, it is not necessary to explain the psychological antecedents of that behaviour. I quote Dr Ian Little ' that one's " ultimate " data should be statistical can no longer be considered a shocking idea. We can, in this respect draw a *good* analogy with physics, in which science the behaviour of electrons is now held to be indeterminate.'[1] So that we need only maintain that no proposition can be directly useful in explanation unless it can conceivably be falsified. This is not to say that the methods of strengthening or falsifying economic propositions are simple: only that we should not so frame our propositions as to exclude, from the start, any possibility of testing them.

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[1] T. W. Hutchison, *The Significance and Basic Postulates of Economic Theory*, London, 1938

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[1] Moritz Schlick, quoted by Toulmin, *The Philosophy of Science*, London; 1953, p. 83

[2] Kaufmann, ' On the Postulates of Economic Theory, ' *Social Research*, 1942, 9, 388

[3] F. H. Knight, ' The Significance and Basic Postulates of Economic Theory : A Rejoinder, ' *Journal of Political Economy*, 1941, 49, 752.

[4] Quoted by Hutchison, op. cit. p. 184

[5] Knight, ' "What is Truth ", in Economics?, ' *Journal of Political Economy*, 1940, 48, 15

The conclusions of Professors Knight and von Mises may perhaps depend on a fourth type of confusion; a failure to distinguish between two types of hypothetical imperative, that is, statements of the type, 'if you want *x*, you must do *y*'. We might divide such statements into two classes, the analytic-hypothetical and the synthetic-hypothetical. If, for example, we were to make freedom from internal parasites part of our definition of health, then the imperative, 'if you want to be healthy, you must avoid ingesting parasites', is an example of the first type. 'If you want to be healthy, you should boil your drinking water' is an example of the second type, since its validity depends on the testable proposition that unboiled water is likely to contain live parasites. The hypothetical imperatives of economics (particularly Welfare Economics) can usefully be distinguished in this way. Pigou's basic proposition for instance, is that in order to maximise the National Income, taken as a convenient measure of material welfare, economic resources should yield equi-marginal net social products in all uses.[2] This is an excellent example of an analytic proposition on which an analytic-hypothetical imperative might be based; it says, in effect, that if it is not advantageous to shift any of our resources out of their present uses into others, we are in an optimal position. It is by no means a useless formulation since by showing that to maximise the National Income *is* to conform to this principle, Pigou gives us a valuable standard against which to assess existing arrangements and proposals. A statement like 'free competition will maximise economic welfare' is, however, synthetic and can serve as the basis only for a synthetic-hypothetical imperative. We might try to test it by discerning whether the spur of competition or the lure of monopoly profits leads to the higher output. Some economists have been guilty, perhaps unconsciously, of smuggling in all the propositions they wish to make in Welfare Economics under the umbrella of useful tautologies of the Pigovian type.

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[1] I. M. D. Little, *A Critique of Welfare Economics*, Oxford, 1950, p. 52

[2] A. C. Pigou, quoted in I. M. D. Little, op. cit. Pt II, chap. 3

The most extreme, persistent and able of the exponents of the view that the whole of Economics is essentially *a priori* is probably Professor von Mises. He refers often to a special kind of knowledge which he calls ' apodictic certainty '.

I have failed to discover that he anywhere uses the phrase 'synthetic *a priori* propositions ', but, for all that, his position plainly derives directly from Kant. To summarise his general position, I quote: ' The science of human action that strives for universally valid knowledge is the theoretical system whose hitherto best elaborated branch is economics. In all of its branches this science is *a priori*, not empirical. Like logic and mathematics, it is not derived from experience; it i prior to experience. It is, as it were, the logic of action and deed.' One of von Mises's disciples, Faustino Ballve, brings out the implicit Kantianism in his own exposition:

> Economics, like any other science, necessarily starts from an *a priori* synthetical assertion. For economics it is the following; economic activity in elective-catallactic action. . . . Starting from the synthetic assertion that defines economics, the science of economics proceeds in an analytical way, searching for fundamental elements, in order to construct the theory of economic life. First, it searches for the primary categories of economic thought. According to von Mises, human action is the fundamental element; but not every action is economic—only catallactic action is. It necessarily concerns commodities and services, even in the case of elective rejection. Being directed to future uses it necessarily entails insecurity, risks, etc.[2]

This general position was followed by Robbins in the *Essay on the Nature and Significance of Economic Science*, although there are a good many hedges there, especially in the second edition, about the epistemological assumptions. He does, however, commit himself to the statement that ' it is only those generalisations which have the character of certainty which are Economic Generalisations in [capital letters] proper '.[3] Examples which he gives are the propositions of value theory, which follow from the assumption that the individual has an ordinal scale of wants; the law of diminishing returns, following from the fact that different factors of production are imperfect substitutes for each other; and the propositions of monetary theory, and of economic dynamics, which follow from the existence of uncertainty.

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[1] L. von Mises, *Epistemological Problems of Economics*, translated by G. Riesman, William Volker Fund. Series in the Humane Studies, Princeton, 1960, pp 12-13

[2] M. Sennholz (ed.), *On Freedom and Free Enterprise*, 1956

[3] L. Robbins, London, 1935

What I want briefly to show is that, in so far as I can understand von Mises's position, it is not, in fact, Kantian. He seems to be sheltering in the wrong trench, and once his position is exposed, and left to stand on its own logical legs, its tenability seems highly doubtful. A synthetic *a priori* proposition, for Kant, was a proposition about the nature of such concepts as space, time, quantity and causality. We are supposed to have '*a priori*' knowledge of these concepts in the sense that no empirical knowledge, or even orderly experience, including our knowledge of material objects, is possible without them; they are logically prior to all experience. The knowledge is also 'synthetic', because it is revealed only *through* experience, e.g. specific cases of cause and effect, not by mere reflection on the necessary properties of a concept which we have devised in a certain way. It is important to my argument that I should be permitted to follow the majority view of Kantian expositors and maintain that this is fundamentally a *logical* argument, not a psychological one, as Pritchard, for example, seemed to think. Kant does sometimes write in a rather 'psychological' way, saying for example, that time is 'nothing but the subjective condition under which alone intuition can take place in us.' But since he holds that it is impossible to conceive of objective experience as timeless—he refers by contrast, to the cases of disordered perception or of dreams—he is really concerned with the fundamental categories of *knowledge*, not with empirical facts about *psychology*, where different psychological principles would be at least conceivable. Now can von Mises's or Robbins's propositions be held to be synthetic *a priori* in this sense? It is not too easy to discover precisely what von Mises's fundamental propositions are, but two important ones appear to be that all human action—as distinguished from mere physical behaviour or what he sometimes calls 'vegetative reaction'—is *purposeful* (he usually uses the term 'rational', but in a special sense) and that the general purpose underlying all action is to remove 'uneasiness' or 'dissatisfaction', to exchange a less desirable state of affairs for a more desirable one. (Economics is about that special class of action where the conditions of the two states which are exchanged can be fairly thoroughly described in terms of 'possessions', which are measurable on the same scale, using possessions in the widest sense to include such things as 'leisure'.)

Such propositions do not appear to belong to the category of Kant's 'synthetic a priori'. They may indeed be *a priori* because they depend on a particular definition of action; von Mises writes, for instance 'The statement "he acts irrationally" is meaningless, because it is not compatible with the concept of action'.[1] But that would make them analytic propositions, i.e. truths of definition alone; von Mises requires that they be *synthetic*, since he wishes these propositions, or their consequents, to be empirically informative, to tell us about the real world. As Robbins puts it, economic laws 'are distinguished from the propositions of pure logic and mathematics by the fact that in some sense their reference is to that which exists, or that which may exist, rather than to purely formal relations'.[2] But von Mises's insistence on the impossibility of subjecting his propositions to any test, which I shall return to in a minute, seems effectively to debar them from having any such reference to empirical reality. I think that what he, in fact does, is to develop Kant's notion of the synthetic *a priori* in what I have argued to be the illegitimate way, i.e. as if it were a psychological concept. He writes, for instance,

> What we know about the fundamental categories of action—action, economising, preferring, the relationships of ends and means . . . is not derived from experience. We conceive all this from within, just as we conceive logical and mathematical truths, *a priori*, without reference to any experience. *Nor could experience ever lead anyone to the knowledge of these things if he did not comprehend them from within himself.*[3]

This last sentence, and the flavour of much else of his writings, sounds as if von Mises is really equating *knowledge derived from introspection* with *a priori* knowledge. This is also Professor Hutchison's interpretation, reached with the advantage which I lack of being able to read von Mises in the original German. This general idea that the social sciences are in a particularly advantageous position to achieve certain knowledge, because their fundamental theories depend on introspection, whose results are held to be intuitively certain, is of long standing. Taking it back only as far as the eighteen-seventies, Cairnes wrote,

> The Economist starts with a knowledge of ultimate causes . . . they consist of such facts as . . . certain mental feelings and certain animal propensities in human beings; . . . For the discovery of such premises no elaborate process of induction is needed. In order to know, *e.g.*, why a farmer engages in the production of corn, why he cultivates his land up to a certain point, and why he does not cultivate it further, it is not necessary that we should derive our knowledge from a series

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[1] Von Mises, op. cit. p. 33

[2] L. Robbins, op. cit.

[3] Von Mises, op. cit. pp. 13-14

> of generalisations proceeding upwards from the statistics of corn, and cultivation, to the mental feelings which stimulate the industry of the farmer on the one hand and, on the other, the physical qualities of the soil on which the productiveness of the industry depends. It is not necessary to do this (because) we have, or may have if we choose to turn our attention to the subject, direct knowledge of these causes in our consciousness of what passes in our own minds. . . . Everyone who embarks in any industrial pursuit is conscious of the motives which activate him in doing so.[1]

And, again,

> No one thinks of framing an hypothesis as to the motives which induce men to engage in industry, to prefer remunerative to unremunerative occupations, or to embark their earnings in investments which, *ceteris paribus*, promise the best returns; . . . any more than as to those on which depend the continuance and growth of population. Conjecture here would manifestly be out of place, inasmuch as we possess in our consciousness . . . direct and easy proof of that which we desire to know.[2]

I think that such passages may be as pernicious as they are elegant; partly because the leading idea seems to me wrong in itself, partly because it has had the unfortunate consequence of shunting economics off the main line of its advance for too long. I think it is untrue that we can derive from introspection any sort of certain knowledge about economic behaviour; even of the existence of what Robbins calls an hierarchy of ends. I will take the extreme view and maintain not only that we have no grounds for generalising to all men our introspectively gained knowledge of our own behaviour, but also that we do not get much certain knowledge of ourselves from introspection, or at least not knowledge about the causes of our actions. If we come to the conclusion that we ourselves do have a consistent ordinal scale of wants, or ends, we perceive this from our own behaviour, not from an armchair study of our states of mind. Suppose we were asked, for example, whether we consistently valued foreign travel more highly than living space. We do not try to answer such a question by turning ourselves into juke boxes, as it were, inserting first the stimulus of the phrase 'foreign travel' and then that of 'living space' and seeing which plays louder or more pleasantly. We consider how we have actually apportioned our expenditure in the past. Or we may consider how we plan to apportion it in the future—but, even so, knowledge of our own previous behaviour is highly relevant, if only to inform us how closely our present intentions are likely to conform with our actual behaviour. In other words, we answer such questions about ourselves with precisely the same kind of evidence we use in answering them about others. The advantage we have when we are ourselves the subject of our enquiry is simply the more abundant knowledge of our own behaviour, rather than any special *kind* of knowledge. I am not, incidentally, saying that our knowledge of mental states can *only* be obtained from externally observable behaviour. But I do not think we can derive useful knowledge about the behaviour of men in a situation of choice from the sort of self-interrogation which tells me whether or not my gratification at having been asked to read a paper to this audience outweighs my trepidation about the discussion which will follow. As far as our work as economists is concerned, I would incline to agree with Ian Little, even against such staunch champions of commonsense as Professor Robertson, that there is no escape from being behaviourist in our psychology.

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[1] J. E. Cairnes, *The Character and Logical Method of Political Economy*, 2nd edn., London, 1875, pp. 75-76

[2] J. E. Cairnes, op. cit.

The unfortunate consequence which this reliance on the supposed special status of introspective knowledge has had is, of course, to have put economists off doing empirical work that needs to be done. Going back to Cairnes, it just isn't true that we have ' direct knowledge in our consciousness ' of ' why a farmer engages in the production of corn '. To find out why may need quite elaborate sociological and economic investigation. The situation as regards opportunity costs, even in monetary terms, is seldom crystal clear; even if it were, a large field of enquiry still remains as to what factors, including the non-monetary, enter into the opportunity costs which the farmer actually considers; and as to how far farmers think in terms of opportunity costs, or of conscious choice, at all. J. R. Bellerby's valuable book, *Agriculture and Industry: Relative Income*[1] is, in part, an attempt to solve these questions, an attempt which would not have been necessary had we been able to spin the answers from armchair introspection. Again, if our knowledge of the fundamentals of human behaviour were so easily gained, economists would not have had to engage in such strenuous argument as to whether or not the peoples of poor countries respond easily to economic incentive. The only way of settling this and similar questions is to go and look. In fact Cairnes may well have meant only that it was a sensible procedure, in these matters, to take money-maximising behaviour as the *norm*,

[1] London, 1956

and to treat departures from it as calling for special investigation; rather as, with the laws of supply and demand, we treat, say, increased consumption to be the *normal* reaction to a price fall and regard other reactions as needing special investigations. But von Mises wishes to translate this position into something much more dogmatic and, to use a phrase of A. L. Lindsay's, ' high priori '. My own view is that there are only three useful procedures for the economist; first, to set up hypotheses, and here introspection may play a very important rôle in suggesting them; second, to use logic, including mathematics, to tease out the consequences of these hypotheses; and third, to examine the external world, by whatever methods which the particular aspect under study dictates, in order both to test existing hypotheses and to set up new ones. It is only in the second of these procedures, using logic, that certainty can be guaranteed, and it is obscurantist to suggest that the economist possesses any secret weapon that will confer ' certainty ' on results gained by any other means—that is that he can, by other means, produce results which are exempt from the necessity of testing.

One final consideration to try to remove von Mises's Kantian prop: it is true that Kant considers the concept of causality to be a part of our synthetic ' a priori ' knowledge—synthetic because we learn it only from our observation of specific instances of causality; *a priori* because what we know as objective experience is unthinkable without it. So it might seem as if von Mises's view that our knowledge that all human action is purposeful, or motivated, is also ' synthetic *a priori* ' comes to the same thing. But I don't think it does: first, when Kant talks of causality he means much what Hume meant by ' constant succession ', *not* an explanatory principle; his difference with Hume is that he insists that universal causality is a necessary condition of our knowledge, not just an hypothesis mistakenly derived from the constant association of ideas. So that on this ground alone he is rather unlikely to count *motives* or *purposes* as ' causes ', or at least as more than one factor in a causal complex. He would be more likely to enquire ' what *made* you do that?' than ' why did you do it?', or ' with what purpose did you do it?' Second, he holds that, from a ' phenomenal ' (quote) point of view, all human actions are determined, which doesn't seem to square with von Mises's view of human action as ' elective-catallactic ', which has a strong overtone of free choice about it. In other words, if von Mises is relying on Kant at all here I think he is falsely assuming that ' cause ' in this context means ' motive ', whereas I would hold that we have to stretch the meaning of 'motive' rather far, to include, e.g. unconscious 'motives', before we can treat the two as even significantly over-lapping.

If von Mises's fundamental propositions cannot rightly lay claim to the grandeur of Kant's 'synthetic a priori' statements, is there anything to which they can lay claim? He seems, in many cases, to treat them as truths of definition, or as analytic propositions—as, for example, 'The statement "he acts irrationally" is meaningless, because it is not compatible with the concept of action'. This means, of course, compatible with *von Mises's* concept of action, since, in ordinary language, we certainly can speak of irrational action, and make ourselves understood; and even if we translate 'rational' as 'purposive', it makes perfectly good sense to talk of purposeless action. So that all that von Mises has succeeded in conveying here is that when *he* uses the phrase 'human action', we must understand action intended to serve the general aim of removing, or lessening, dissatisfaction. This statement is therefore unfalsifiable, since to point to examples of what *we* consider to be actions, and say that they don't seem to fall into this category, is irrelevant. But the statement can be fairly held to be misleading, on either of two counts. If his use of 'action' is in fact rather far from its common usage, then it is unfortunate that he uses the same word—this is a common difficulty in economics, as with such words as 'market', 'rent' and so on, but with these latter examples, at least there is usually a commonly established *economic* usage. If, on the other hand, von Mises is really saying something illuminating; that, on reflection, observation and perhaps experiment, we would agree that all human 'actions', in the ordinary sense, *can* be said to aim at removing uneasiness, then he is propounding a psychological law, which can only be confirmed by the empirical techniques appropriate to psychology. So that his way of treating his proposition becomes misleading because it is made to depend on definition when it does not. If this criticism is true of the fundamental proposition of 'praxeology', it is true for all the propositions that von Mises claims to deduce from it.

In fact, von Mises's position is completely consistent. He does hold that no constellation of empirical facts can either confirm or falsify economic theories. He writes, for instance, 'when arguments based on principle concern questions of action, one should always be ready to admit that nothing can be found more dangerous and unworthy of a philosopher than the vulgar pretension to appeal to an experience to the contrary'.[1] The supporting argument is that 'complex phenomena in the production of which various causal chains are interlaced cannot test any theory. Such phenomena, on the contrary, become intelligible only through an interpretation in terms of theories previously developed from other sources'—the 'other sources', here, are presumably introspection and logic. Many economists have shared his view, among them Frank Knight and possibly Professor Robbins, and it has obvious links with the disparagement of quantitative economics which the same writers usually reveal. But I feel it makes an unwarrantable distinction of principle between the social and the natural sciences. Of course it is very difficult to make the necessary isolations from social situations, but I doubt whether success is completely impossible and, if it were, there would seem to be no justification whatsoever for the economic profession. I am incompetent to speak intelligently of new mathematical and econometric techniques, but I have the impression that some of them have made significant contributions to this problem of isolation, and may eventually also contribute to the problem of prediction. Von Mises himself appears to perform the operation he calls impossible when he insists that the growth of material prosperity has been fundamentally due to the unhampered private ownership of the means of production.

I hope that my paper, so far, has not seemed too much like amateur and irrelevant epistemology; it is certainly amateur, but, in this field, von Mises has not lost his status either, so that the contest may be lost, but not declared void. And I think it is highly relevant because of the link between the insistence on the 'a priori' character of economics and the apologetic for *laisser-faire*. If it is impossible to discover empirical regularities in economic affairs, they are not suitable subjects for prediction, and if we cannot predict the effects of any induced change, we might as well let moderately well alone. I have tried to argue that in so far as von Mises does make his *a priori* case he does it only by relying on certain private conventions of language, and that we have no obligation to follow these. If we *do* decide to follow them, it will be because empirical investigation tells us that 'human action', etc., is, in fact, rather like what he defines it as. But then our propositions will be frankly synthetic, open to being falsified by experience, and thus a suitable basis for an empirical study in which the case for or against *laisser-faire* can in principle be decided on an analysis of facts, not of concepts.

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[1] Von Mises, op. cit. p. 29

I hope I may have succeeded in weakening the extreme *laisser-faire* position by depriving it of its twin claims to rest on irrefutable logic *and* to be about the real world. I would adapt Einstein here: 'As far as the theorems of economics refer to reality, they are not certain, and as far as they are certain, they do not refer to reality.' Von Mises attempts to refute the applicability of this quotation by saying that our knowledge of human *action* is both empirical and certain because action is an *offshoot* of reason, and hence our reason has a special relationship to it, guaranteeing certain knowledge. But this seems to be using 'reason' in two senses. In the first sense, action is held to be 'reasoned' because, by definition, it consists in exchanging a less for a more satisfactory state of affairs: this sort of 'reason' could be possessed by animals and psychologists seem to find it useful to assume that it is. In the second sense, reason means the power of reaching correct theoretical conclusions, which is an entirely different matter. If the *laisser-faire* position loses these fundamental supports, it must submit itself to some sort of empirical test. Even to say 'this is the arrangement I like best' is to make an *empirical* statement; it would be possible to show that I was mistaken in what I thought I liked. If economic arrangements are to be submitted to some sort of greatest-happiness-of-the-greatest-number test, this is again an empirical matter. What makes most people happy—that is, certain people at a certain time? I don't think this question is unanswerable and the *laisser-faire* protagonists indeed give it at least an implicit answer; the availability of a relatively wide choice, and large quantity, of purchasable goods. They leave out of consideration any satisfactions desirable from living in a certain type of society, whereas I would wish to include them. Their inclusion might, of course, tell against my own roughly egalitarian position if it were shown that people in fact preferred the excitements of an inegalitarian society. Questions of coercion, income re-distribution, i.e. altering the initial distribution caused by the productive process, public ownership of industry, etc., then also become a matter of empirical enquiry, and cannot be ruled out of court as self-evidently stupid or self-contradictory.

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DISCUSSIONS

ESP AND THE ‘THEORY OF RESONANCE’

Dr Ninian Marshall's theory[1] proposed to account for telepathy illustrates in what I regard as a typical fashion the very serious difficulties to which all such speculation is exposed. Alleged ‘physical theories’ of ESP succeed in becoming plausible only by ignoring just those features of ESP which are most difficult to fit into the current cosmological, quantum-mechanical and informational-theoretical approaches. Since the difficulties are far more serious than most orthodox scientists and most orthodox psychical researchers have tended to suppose they are, I should like to show how theories of this kind fare when we consider the *details* of alleged cases of ESP.

Marshall claims to have met the problem of the spatio-temporal separation of the ‘ESP percipient’ and the ‘target’ with a ‘holistic law’ making what is virtually ‘noise’ available as ‘information’. He contends that the possibility is admissible in the context of present-day quantum theory and refers to some analogous considerations adduced by W. M. Elsasser.[2] Elsasser has argued that intrinsic uncertainties of measurement of which quantum theory and information theory both speak are cumulative in a fashion which renders dubious the familiar methods of analysing complex biological wholes in terms of simpler constituents. Just here crucial difficulties arise. A reviewer[3] has said about Elsasser's suggestion that ‘the reader is . . . likely to be left quite frustrated about finding any clue to the nature of the “biotonic laws” or how to go about seeking for them.’ Do Marshall's laws promise more for psychical research? Eugene P. Wigner[4] finds that Elsasser's argument involves the contradiction of supposing, on the one hand, that ‘biotonic laws’ are not contained in the laws of quantum mechanics and, on the other hand, that the latter are not negated by the new laws. Wigner constructs a quasi-mathematical argument to show that ‘biotonic laws’ are in principle not derivable from quantum mechanics and that, therefore, in all probability, ‘present laws and conceptions of quantum mechanics will have to undergo modification before they can be applied to the problem of life’. He has voiced his ‘firm conviction’ that the existence of ‘biotonic laws’ stems from the ‘overwhelming phenomenon of consciousness’. May not this hold for Marshall's ‘holistic laws’ as well?

Philosophical issues about quantum mechanics are far from unambiguous judged by the still strongly divided opinions among experts.[5] Even the role of ‘observation’ in quantum theory continues to be contested hotly.[6] Can we be sure that

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[1] This *Journal*, 1960, **10**, 265-86. [2] *The Physical Foundation of Biology*, London, 1958

[3] Manfred Kochen in *Information and Control*, 1961, **4**, 232

[4] E. P. Wigner in *The Logic of Personal Knowledge*, Essays presented to M. Polanyi on his Seventieth Birthday, Glencoe, Illinois, 1961, Ch. 19

[5] See, for instance, *Current Issues in the Philosophy of Science*, ed. H. Feigl and G. Maxwell, New York, 1961

[6] See, for instance, Hugh Everett, *Reviews of Modern Physics*, 1957, **29**, 454-62; and the comments by H. S. Green, *Il Nuovo Cimento*, 1958, **9**, 880-9; R. Kawabe, ibid., 1959, **13**, 448-50; A. Komar, *The Physical Review* (2), 1962, **126**, 365-9; and J. M. Burgers, *Reviews of Modern Physics*, 1963, **35**, 145-50

Marshall's 'hypothesis of resonance' is physical in the sense that it excludes or minimises the direct intervention of psychological variables? The 'inputs' which supply the 'ESP messages' may well be purely psychological if the individual character of the 'messages' is withdrawn from direct observation.

The objection assumes more serious proportions when we consider the details of the telepathic cases in the alleged spontaneous life-situation. The problem is far from being so straightforward that it can be summed up by saying that a brain state ϕ associated with a mental state ψ in the person *A* evokes by 'resonance' an analogous brain state ϕ' associated with a mental state ψ' in the person *B*. In many spontaneous cases of telepathy there seem to be available to the alleged 'telepathic percipient' unexpected and inconspicuous details of the dress (e.g. rolled-up trousers showing the lining, onyx shirt-studs, a small hole in a petticoat) and the environment of the alleged 'telepathic agent'. It is doubtful in these cases whether the 'agent' was aware of the details at all. Sometimes there seems to be a surprisingly appropriate reversal of right-left orientation. The 'agent' may be looking at a small antique lamp on his *right* in his study; but to the 'percipient' the lamp appears on his *left* as it would if he were normally face to face with the 'agent' and looking at objects *not* in the normal 'field of vision' of the 'agent' at all. There seem to be also cases in which the alleged 'telepathic agent', 'percipient' and even bystanders (not necessarily twins, cousins or even blood relations) *all* become 'percipients' viewing the interpersonal situation from seemingly appropriate 'spatial' viewpoints. Hornell Hart, formerly of the Department of Sociology, Duke University, has made a special study of these cases of 'ESP projection', as he calls them.

Information about spatial attributes even in the simplest receptor organ, e.g. a Pacinian body has to be conveyed by fairly elaborate methods to the brain.[1] The channels along which visual and auditory information is conveyed are more intricate.[2] All aspects of spatial orientation, two-point discrimination, postural sensibility, position sense, stereognosis, autotopognosis, right and left discrimination, depend on dynamic information involving the power of the cortex to select subtle and varying patterns of motion.[3] So far as I am able to judge, Marshall's 'theory of resonance' fails to account for the appropriately differentiated patterns of the 'veridical hallucinations' in cases of 'collective telepathic percipience'. Marshall ignores, too, for his theory-building, cases of telepathic percipience embedded in what is known as 'object-reading' or 'psychoscopy' and cases of presumed 'telepathic exchange' between man and animal. Can a 'holistic law' make material objects, animal brains and human brains all alike potential sources of ESP information without degenerating into a complete *ignava ratio*?

One hears occasionally about inscrutable 'psi-fields' interacting with 'physical fields' and providing 'ESP information'. R. E. Peierls[4] has said about the far more rigorous domain of nuclear research: '"Field theory" is at best some guide as to what is or is not plausible. We might wait for the happy day, but it may be a long time.' If, in this more orthodox area of investigation, 'we are at the moment forced back to a phenomenological treatment, fitting potentials to the empirical evidence', may it not be more than prudent and advantageous to adopt, for the present, the exclusively phenomenological policy of fitting relevant psychological variables into the evidence for ESP? Quantum field theory is in none too incontrovertible a position [1] to permit safe analogies to be drawn by psychical researchers.

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[1] See *Progress in Biophysics and Biophysical Chemistry*, ed. J. A. V. Butler and B. Katz, London, 1959, Ch. 6 [2] Ibid., Chs. 1 and 2

[3] See W. Gooddy and M. Reinhold, *Brain*, 1953, **76**, 337-363; Eberhard Bay, ibid., 515-55

[4] *Proceedings of the International Conference on Nuclear Structure*, Toronto, 1960, p. 11

Precognitive telepathy directly suggested by the experiments (S. G. Soal's investigations with Basil Shackleton and Gloria Stewart) to which Marshall has referred is a crux for all theory-building, especially when an informational-theoretical approach is adopted. L. Brillouin's researches [2] suggest that only what is *past* in relation to a 'here-now' can be informationally *recorded.* The suggestion is massively supported by all the informational-theoretic researches so far carried out in biology, e.g. into genetic noise and redundancy, survivor-curves, and information content of organelles, 'biological time' and biotopology.[3] Against all theoretical physicists who would make an irreversible time a psychological convention, J. M. Blatt [4] has argued forcibly that the distinction between 'past' and 'future' is made by inanimate recording devices as well as by animate observers. The circulating magnetic 'memory traces' of the modern computer bear an analogy to the 'memory traces' of the vertebrate nervous system. Even on Marshall's theory, there can be 'resonance' only with the 'past'. The best linear and non-linear filters and the whole of the prediction theory fail to provide any relevant analogy to precognitive ESP.

Elsasser and Wigner have argued that the high reliability of the Watson-Crick models in cytogenetic phenomena presumably depends on 'biotonic laws'. Elsasser hints at 'internal inputs that supply out of an immense reservoir of the developing organism'. He maintains that the increase in information is not primarily related to interaction of the organism with the environment. Precognitive ESP confronts us with the paradox that *specific* information about the *environment* may be obtained in the absence of any interaction with it we can understand in the whole context of present-day information theory. Or, to state the problem in another way: Elsasser's 'biotonic laws' attempt to state structured properties of the developing organism essentially as functions of an asymmetric or irreversible time. Precognitive ESP flouts all our ordinary notions of time-ordering.

In this predicament, we may cast about for 'world-models' in which there are 'time-like' lines stretching from 'future' to 'past'. I have argued elsewhere [5] that even these desperate remedies fail to solve the problem of precognition. Wiener[6] has argued on informational-theoretical grounds that an observer whose time runs in the opposite direction to ours could not possibly 'convey information' to us. Can a brain or a 'subconscious' with 'reversed time-tracks' help to 'exteriorise' a precognitive insight as an 'anticipatory memory'? How is the 'reverse information' compounded with the normal 'forward information' to yield a precognitive insight on which the percipient sometimes *acts*? O. M. P. Bilaniuk, V. K. Deshpande and E. C. G. Sudarshan[1] have argued that a 'reversal of time' for a particle or a Lorentz transformation signifying a change of the sign of energy may conveniently be interpreted as a change in the description of the process. By an appropriate choice of the frame of reference, we say, for instance, that the particle is 'absorbed' instead of 'emitted'. The argument seems to be confined to Lorentz invariance and particles. It sheds no conceivable light on precognition occurring in an ordinary terrestrial setting in which neither the 'direction of time' nor memory can be regarded as a trivial choice by a *single* observer of a 'frame of reference'. R. W. Fuller and J. A. Wheeler[2] have shown that, with a relativistic description, even a multiply-connected topology of space-time may not provide for a particle travelling with the speed of light outpacing another particle of equal speed by taking a shorter path through a 'wormhole'. Some enthusiasts[3] have suggested that space-time description is fatally macroscopic and must be renounced in the microphysical world. It is impossible to extract a detailed hypothesis about ESP from so vague and sweeping a suggestion. I maintain that no physical theory of ESP proposed so far has successfully or even plausibly accounted for *all* the principal known facts about telepathy, clairvoyance, and precognition.

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[1] See, for instance, H. P. Stapp, *The Physical Review* (2), 1962, **125**, 2139-62; *Reviews of Modern Physics*, 1962, **34**, 390-4; P. A. M. Dirac, ibid., 592-6

[2] L. Brillouin, *Information and Control*, 1959, **2**, 45-63

[3] See the survey by H. P. Yockey in *Radiation Biology and Medicine*, ed. W. D. Claus, Reading, Massachusetts, 1958, Ch. 11

[4] *Mathematical Reviews*, 1963, **26**, 428; Cf. J. M. Blatt, *Progress of Theoretical Physics* (Kyoto), 1959, **22**, 745-56

[5] 'Time Reversal, Information Theory and "World-geometry",' *J. Philosophy*, 1963, **60**, 579-83

[6] *Cybernetics*, second edition, New York, 1961, pp. 34-35

C. T. K. Chari

Note on Goodstein's 'The Significance of Incompleteness Theorems'

In 'The Significance of Incompleteness Theorems' (this *Journal*, 1963, **14**, pp. 208-220) R. L. Goodstein contrasts the classical 'incompleteness theorems' of geometry (and, he should have added, algebra) with the foundational results which stem mainly from the work of Gödel and Löwenheim. He elucidates this latter work expertly and indicates both the source and the error of several common misinterpretations. In doing so, however, he pays scant attention to the classical results and not only states them incorrectly but makes a hasty evaluation of their significance. Goodstein asserts (p. 208) that an axiom system for projective planes which consists of the incidence axioms plus Pappus's theorem is incomplete, because Desargues's theorem is undecidable in the system. But it is the remarkable (and well known) result of Hessenberg (*Math. Annalen* 1905, 61) that in the presence of the axioms of incidence, Pappus's theorem implies Desargues's theorem. What Goodstein may have meant is either that (1) Pappus's theorem is undecidable on the basis of the incidence axioms and Desargues's theorem, or that (2) Desargues's theorem is undecidable given only the incidence axioms. This brings us to Goodstein's evaluation, which is simply to deny the philosophical importance of these geometrical 'incompleteness theorems'. While it is true, as Goodstein points out, that the incompleteness discovered by Gödel *et al* differs from (1) and (2) in being essentially unremovable (for sufficiently rich, recursively axiomatisable systems) and while, as Goodstein correctly remarks, this fact is overlooked by some of those who deprecate these foundational results, it does not follow that (1) and (2) have little or no philosophical interest.

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[1] *Amer. J. Physics*, 1962, **30**, 718-23

[2] *The Physical Review* (2), 1962, **128**, 919-29; Cf. J. A. Wheeler, *Reviews of Modern Physics*, 1962, **34**, 873-92

[3] See, for instance, E. J. Zimmerman, *Amer. J. Physics*, 1962, **30**, 97-105

Anyone who is interested in the foundations of mathematics, whether from mathematical or epistemological motives, must come to understand how the connection (exploited by Descartes) between algebra and geometry is possible. How does it happen that one can do geometry via algebra, that one can co-ordinatise a geometric plane, and does it always happen that one can do so? Here Desargues's theorem provides the requisite condition for obtaining a ' nice ' (i.e. associative) coordinate set and (1) demonstrates that even then, the coordinates are not nice enough. For only Pappus's theorem (or some equivalent) guarantees that the multiplication of coordinates is commutative. Thus (1) and associated results provide a startling and profound insight into fundamental aspects of geometry. Since philosophical interest in geometry has roots which go back at least as far as the interest in logical systems, it is difficult to see why Goodstein peremptorily dismisses the former in favour of the later.

The Skolem paradox is significant, as mentioned by Goodstein, because it has generated a whole area of research into nonstandard models and some concomitant philosophical discussion of the relation between a formal system and the subject matter and objects it deals with. But (2), the undecidability of Desargues's theorem, has similarly generated one of the most active fields of research in geometry : the discovery and classification of non-Desarguesian planes. This activity too has been accompanied by a good deal of discussion of the ' ontological ' status of geometric objects and it has illuminated the older discussions of Euclidean vs. non-Euclidean geometry. Again, Goodstein's neglect seems unwarranted.

None of the foregoing is intended to disparage what Goodstein does say about the logical ' incompleteness theorems ' it is only intended to correct a misleading impression and to indicate, for it is not appropriate in such a note to amplify, what Goodstein could have said about the more classical results.

ARTHUR I. FINE

REPLY TO MR FINE'S NOTE

IT is true that I overlooked a misprint in the third line of the second paragraph of my paper where the first word should be ' without ', not ' and '.

It was not my intention to undervalue the importance of the proof of independence of Desargues's theorem and I accept the criticism that I could have said more about the concept of independence.

R. L. GOODSTEIN

REPLY TO G. A. BARNARD

MY article[1] does not ' hinge ' on the statement singled out by Professor Barnard. This is admitted by him, apparently unconsciously, when he goes on to serious discussion of my distinction between each and all. The statement in question is a provocative aside, and I am glad Professor Barnard has responded.

If we put any notion of non-enumerability out of our minds, Skolem's theorem (also König's proof of 1905) shows that the real numbers are enumerable. Barnard's position is similar to that of veterinarian B who objected to A's findings on the horse because A had not considered the unicorn. My article pointed out a defect in the optical instrument used in unicorn sightings. To translate, all proofs of the existence of the transfinite rely on a simple contradiction in terms, namely, there is an end to an infinity. Since any approach to the transfinite must get beyond the enumerably infinite, it is at least as clear to me as Hilbert and Bernays's *Grundlagen* is to Barnard that there is no transfinite. After all, even metamathematicians are now forced to admit that their proofs and beliefs are ultimately intuitive. And how long did chemistry believe, with considerable success, in the existence of the undissociated molecule in electrolytic solutions before realising there is no such animal? It is not that, in the delightful phrase of Dr Lakatos, I am barring Barnard's unicorn because it is a monster—I am barring it because it does not exist within mathematics.

R. J. Diamond

[1] 'Each and All,' this *Journal*, 1964, 14, 351.

REVIEWS

THE REVOLUTION IN ANTHROPOLOGY

I

PHILOSOPHERS who write about the work of social scientists are often criticised by them for their ignorance of current problems and current results. Philosophers deal exclusively, it is said, with yesterday's problems; and this deplorable exhibition of cultural lag makes it certain that their tardy comment will affect no one who is not equally ignorant. Hence Mr Rodney Needham has urged that critics concentrate on the field in which they are competent, and then gone on to say: 'the clearest and most decisive procedure may be to isolate one typical issue, and to assess the relative efficacy of types of explanation by reference to this. Ultimately, perhaps, the logic of explanation in social science may best be investigated through the practice of explanation.'[1] *The Revolution in Anthropology*[2] by Dr I. C. Jarvie has been written almost as if in response to this suggestion. For it describes one typical problem of social anthropology—that of Melanesian cargo cults—considers the character of the explanations offered by anthropologists, puts forward one of its own, and embeds all this in a vigorous discussion of functional explanation in anthropology, and Malinowskian functionalism (The Revolution) in particular. The product may not satisfy the client, but his recipe has been followed.

Jarvie takes British social anthropology to be committed, at present, to functionalism—to explaining social institutions, categories, and events by indicating their place within a fixed social structure and by specifying their mutual causal dependence within that given system. He distinguishes between (*a*) the original version of Malinowski in which it is assumed that all cultures consist entirely of elements needed for satisfying the biological requirements of people—elements without whose presence the culture could not survive, and (*b*) the modified version of Radcliffe-Brown (' structural-functionalism ') in which a society is thought of as an interlocking set of sub-structures whose task is to maintain the total system in that steady-state called ' social integration '; Radcliffe-Brown provided, as Malinowski did not, for dysfunctional elements, ones that work to make the system unstable or tend to make it disintegrate. Jarvie's main criticisms of both versions are these: anthropologists often use a functional explanation to account for the causes of an institution when they are logically entitled to account only for the effects of that institution upon other social processes and events; since the use of a structural-functional explanation presupposes an unchanging social structure, this type of explanation cannot cope with problems of social change, e.g. the rise of cargo-cults; when functional explanations are satisfactory they are special cases ' of the more general theories that people act according to how they see the logic of the situation, and that many events are unintended consequences of other actions ' (p. xvii).

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[1] *The Oxford Magazine*, 24 October 1963, 38

[2] I. C. Jarvie, *The Revolution in Anthropology*, Routledge & Kegan Paul, London, 1964, pp. xxii + 248. 40s.

Anthropologists have often tried to draw a distinction between pure description (ethnography) and the theoretical synthesis of ethnographic details (comparative sociology). This distinction, says Jarvie, is misleading because based on the false inductivist claim that science proceeds by constructing generalisations from masses of carefully collected facts. Malinowski and Radcliffe-Brown were both victims of this pre-Popperian myth, for both thought of primitive societies as quasi-organic wholes, rather like plant specimens, which first needed to be classified by structure and function, and after thus being organised into classes, could then be the subject of those scientific generalisations which arose, as if by magic, from the data. This attempt to produce a vast collection of social snapshots of different societies—snapshots which could be patiently compared with each other—can only lead to inexplicable correlations between the institutions of various societies. The correlations obtained will be useless because unsupported by a theory; and they will be most obviously useless in the case of social change, for the presence of mere succession explains nothing. As against this misguided effort to distinguish theory from pure description, Jarvie holds that since ' all alleged statements of fact are theories or interpretations ', the important difference is that between two types of problems: local problems which have the form 'What explains the behaviour of these people here? ' and comparative problems of the form 'What explains this type of behaviour wherever it occurs? ' Malinowskian functionalism can answer only the first sort of question, whereas what Popper calls ' situational logic '—exhibiting the means by which the agents have tried to achieve their goals in the light of their beliefs and situation—can deal with both sorts.

After these general remarks on anthropological method, Jarvie devotes the four central chapters of his book to discussing cargo cults as an instance of social change, the various difficulties he finds in the explanations given of them by anthropologists, the methodological source of these difficulties, and his own suggestions for a satisfactory, whether or not correct, theory. A cargo cult, as Jarvie describes it, has a number of striking features: it originates in the revelation received by a prophet who then leads a group of believers in preparing for the millennium of the near future; this millennium is to consist of the arrival of ships and aircraft bearing European goods for the believers, the ejection of Europeans and the reversal of the colonial power hierarchy; these beliefs are often accompanied by ritualistic aping of European customs and procedures, collective hysteria, and destruction of material goods; the cults usually arise in an isolated, economically impoverished, and politically decentralised area which has been subject to Christian missionising. Jarvie rejects such explanations of the cults as those in terms of the prophet's demagoguery, the desire for a new religion, economic hardship, colonial oppression, the rise of nationalism, ulterior aims, and retreat into fantasy. He dismisses them on various grounds, the chief being that they do not allow the explicandum to be derived from the explicans, they are not testable, and often not illuminating. To label the cults as irrational is simply to give up hope of explaining them. In any case, the mere fact that the believers' means are inappropriate to the ends they pursue does not make cultists irrational—for they do not know that their means are inappropriate, neither having been presented with the counter-arguments nor being in a position to appreciate their force if confronted with those arguments.

Jarvie's own view, advanced tentatively, is that cargo-cults represent rational attempts by the Melanesians to solve a problem, that of accounting for the difference in wealth and power between Europeans and themselves. 'The structure of their doctrines indicates what is wanted and they are attempts to provide an explanation of why they haven't got it and a religious prescription for how they can get it' (p. 67). A fully developed theory would answer a host of questions: 'Why do some people join the cult and others not? What sort of person is the prophet? Why are the cults so similar? Why do they rise so quickly? What makes them collapse?' These questions can be given satisfactory answers, Jarvie thinks, only if we are prepared to accept the view that people's behaviour should be explained in terms of their goals, decisions, attitudes, and acts. For then changes in their behaviour, or in their institutions, can be 'explained by analysing the situation of, say, the anonymous individuals who have initiated the chain-reaction of happenings that is the social change, in order to see what factors in their situation persuaded them that one course of action was more appropriate to achieving their aims than another' (p. 111). It is because functional explanations do not do this, but trace the consequences of a situation rather than giving its causes, that such explanations cannot account for the rise of cargo cults. They are reactions to contact with Western society and are 'externally caused change'; a functional explanation merely records their effects without supplying their causes, and no scientific explanation of the cults can be provided in this way.

2

Jarvie's book is a counter-revolutionary polemic and will be judged by its intended victims accordingly. However, a number of his main points seem to be indisputably true: structural-functionalism has been of overwhelming importance in recent British social anthropology; Malinowski's version of functionalism did not admit dysfunctional elements nor did its author always distinguish problems of causal origin from problems of structural relationship; anthropologists, e.g. the American, Franz Boaz, have mistakenly tried to do purely descriptive fieldwork (ethnography); many workers have thought of societies as quasi-organic units whose comparison would yield empirical laws; functional explanations do presuppose a social system and do supply us with the effects rather than the causes of the explicandum; there are intra-community problems and inter-community ones; in a sense, a structural-functional explanation 'is simply a combination of situational logic and tracing the unintended consequences of some action' (p. 222) and the rapid rise and brief life of a cargo cult, as well as the frequency with which such cults appear, do present anthropologists with unanswered questions.

Yet on many important issues Jarvie has not so much argued for a conclusion as drawn attention to his claim and then left it to fend for itself. In part this is unavoidable, since he is relying upon views defended elsewhere by Agassi and Popper. But not only is this practice notorious for leaving its employers caught in the cross-fire between friend and foe, it throws an undue burden upon the reader's sympathy. For example, Jarvie attacks Baconian induction as the art of avoiding error—by the scientist being patient, careful, and certain of his facts before generalising from them. If anthropologists 'let their imaginations run riot the subject could be still more exciting: progress would be made. The thing is not to inhibit your imagination, but to be very critical of its products . . .' (p. 6). The difficulty is to know how to follow this advice. Not any imaginative suggestion will do; it has to be relevant to the problem at hand. If an anthropologist's imagination is to run riot in some relevant fashion, it must still be inhibited by his knowledge of the available evidence. So either his imagination is inhibited after his first attempts or he is not being critical of its products. Of course, what Jarvie wishes to emphasise is that the anthropologists' concern with observation, comparison, sampling, and generalising from 'verified facts' is better suited to testing hypotheses than to generating them. But the force of this is not quite clear. Given Jarvie's belief that there is no such thing 'as a theory-free statement of fact', all our observations and comparisons must generate hypotheses of low level, and these can be used to falsify other hypotheses. The anthropologists' concern with observation and comparison, then, will be his method of testing and generating hypotheses of a certain type, e.g. 'The Shoshone Indians had no legal procedures'. If further observation shows that none of the Indian bands in the Basin-Plateau region had any legal procedures, this result may easily lead to various explanatory answers in terms of the dependence of such procedures upon the existence of a supra-family structure, or on the presence of conflict, in any bands. Thus while it is certainly correct to say, as Jarvie does, that a universal hypothesis cannot be logically derived from a conjunction of singular observation-statements—and so cannot be logically generated by them—it does not follow that observation and comparison are not the best methods for stimulating (generating) the production of relevant universal hypotheses. Whether these methods are inferior to others is a question whose answer will require the support of evidence that is not presented here.

The failure of functionalism to deal with comparative problems is one of Jarvie's main themes, so his distinction between comparative and local problems is of some importance. As an instance of the latter he gives a question drawn from Evans-Pritchard's study of the Nuer: 'What is the function of the leopard-skin chiefs in a chiefless society?' The answer—'They settle feuds'—indicates the intelligible ends which the Nuer are pursuing in their situation. When we know what these chiefs do they are no longer a mystery; their presence required explanation because, says Jarvie, 'we explain what we cannot immediately see to be rational' (p. 22). And he accepts 'the idea of goal-directedness as the criterion of rationality' (p. 132). Local problems are properly answered when we learn what goals are being sought with the means known to the agents. Comparative problems, on the other hand, do not take the form 'What is the *function* of ancestor worship—wherever it occurs?', but 'What is the *explanation* of it?' Questions of this type can employ in their answers 'all sorts of other, macro-, theories, including those of sociology and economics' (p. 35). While a satisfactory functional explanation traces the cause of a social event to some other 'unproblematic event or institution', this procedure cannot be used with comparative problems. For the functionalist every social system is self-contained with respect to explanation and no useful comparisons between systems can be made—or so Jarvie claims.

Now the distinction between local and comparative problems is not as straightforward as it may appear to be at first sight. There are some local problems which do not demand explanations in terms of how one institution is functionally related to other institutions. Jarvie offers the example of the Martian whose puzzlement is relieved when he learns that the strange behaviour of religious people during a drought is simply their way of imploring a Divine Being to send rain. There is no functional explanation here, he says, though there is rational pursuit of a goal. But if this is so, then Jarvie has not given a clear account of the difference between local and comparative problems. For he began by suggesting that two kinds of problems—functional and comparative—are dealt with by social anthropologists. 'On the one hand they can ask for an explanation of custom X in society Y; on the other hand they can ask for a general account of custom X wherever it occurs' (p. 21). Yet since the explanation given the Martian is not a functional one, it is not the answer either to a local problem or to a comparative one. What Jarvie has done is to confuse two distinct sorts of questions: (*a*) 'What are these people here trying to do?' (*b*) 'Why do these people here have this custom?' The answers to the first must state the agents' goals and often their intentions. The answers to the second can be much more varied: they may be historical or functional, or be in terms of causal laws or intentions or dispositional motives. Local problems can only be contrasted with comparative problems if question (*b*) is employed, for question (*a*) has no more general form, e.g. 'What are people everywhere trying to do?' This phrasing cannot be altered to 'What are people everywhere trying to do when their efforts resemble those of these people?' For the question then either becomes the same as that about a custom wherever it occurs or requires the trivial answer 'People everywhere are trying to do what these people are trying to do'.

However, once we take the contrast to be that between explaining the presence of a local custom and explaining the presence of a custom wherever it occurs, it is no longer plausible to hold, as Jarvie seems to, that all local problems can be answered by our specifying what goals the agents' are pursuing with the means known to them. Many customs are followed by people unwittingly or as habits, and many customs are social remnants that have only an historical explanation. Some customs, like having one day a month set aside for a tribal dance, are pursued for their own sake. We can often answer the question 'What are these people doing?' by 'They are following such-and-such custom'; but we cannot always go on to ask 'But why are they following their custom?' and expect an answer in terms of a goal which the agents believe the custom will enable them to reach. In general, the presence of a custom cannot be given an intention explanation. This is one important reason why functional explanations are used instead.

Comparative problems are supposed to exclude the possibility of functional answers, yet the basis of the exclusion is not immediately obvious. Jarvie writes that a functional explanation '*presupposes* an economic *structure* of such and such a kind with such and such links between its institutions' (p. 157). He cites as an example a specific fall in the share index, a fall resulting from the investors' confidence being shaken by a crisis in the nation's balance of payments and a corresponding increase in the liquidity of the investors The index and the balance of payments are said to be functionally related. To this treatment two objections can be made. First, the example given i. not a functional explanation. It cannot be correctly said that the function os the share index is to maintain the balance of payments; nor is the function of the latter to maintain the former. The reason is that neither is maintained in an equilibrium position by a variation in the other. For that to occur both would have to belong to a self-persisting system with negative feed-back, and there is no such system here. They are causally but not functionally related. Second, it is suggested, but not argued, that every social or economic system is so different that the function of any given custom must be correspondingly different in each of them. Hence, we cannot ask for the function of X in any system in which it occurs, but only for its explanation. Now there is no good reason why a functionalist of either kind should hold this view. Ancestor worship might well have the same function in many societies—if it can be said to have a function at all. If it has a function then it operates so as to maintain some other property of the system within a permissible range of values. The property being so maintained might well be the same in a number of societies. Anthropologists have often claimed that there are such properties; whether they exist is an empirical question which can only be answered by investigation.

Jarvie's basic difficulty lies in the treatment he gives to three problems which bear directly on one of his chief theses—that functionalism cannot deal satisfactorily with social change. These three problems are: (i) the structure of a functional explanation and the conditions under which it can properly be used, (ii) the relationship between a functional and a causal explanation, (iii) the extent to which anthropological functionalism uses functional explanations. On none of these problems is Jarvie's view absolutely clear. He provides a variety of examples of what he takes to be functional explanations, including Malinowski's explanation of the Kula ring. His summary of that is: 'The trading was a renewing of ties between communities, the ritual and effort reflected the importance of these ties; the ornaments functioned to express the ties' (p. 40). What is omitted in this summary is any indication that Malinowski thought the trade had a reciprocal causal relation with other institutions, that it maintained them and that they in turn maintained it: in short that the Kula ring trade had a function. By not emphasising this vital point Jarvie is led to treat functionalism simply as explanation which states the effects rather than the causes of an event or process. He then criticises functionalists for advancing as the causes of a situation what are actually its unintended effects, e.g. taking as the causal explanation for the presence of magic its disposition to strengthen belief, morals, and tradition. While Jarvie's distinction is certainly correct, he does not provide much evidence that many functionalists were victims of this confusion. He seems to assume that they were, but this is because he believes that they were attempting to supply a causal explanation for the presence of any given institution. However, their avowed interest was in its contribution to the maintenance of a system, for they took causal explanations of the other sort to demand historical answers which they had no means of providing.

The disadvantages, for Jarvie, of his not concentrating on the question whether the functionalists made appropriate use of functional explanations are, in part, that he leaves unclear the nature of these explanations and their relation to causal and lawlike ones. The other disadvantage is this: nothing much comes of his original attack on functionalism as a way of explaining social change; it amounts to saying that functionalists cannot explain social change that is externally caused, e.g. cargo cults. All he can conclude is that they 'do not explain the intrusion of the change-bringing agency in the first place ' (pp. 158–159). But what is it that requires explanation here? Why culture contact took place? Or why it produced cargo cults in some places and not in others? If it is the latter, as it presumably is, then the internal conditions which are the part-causes of the cults can certainly be identified by functionalists. They can also trace the effects of the cults within societies. The performance of neither of these tasks need establish that the cults have any function, and so a functional explanation of them may not be available. On the other hand, if it can be established that the cults have a relation of reciprocal maintenance with other institutions of a self-persisting system, a functional explanation of the cults can be given. The fact that culture contact is one of their part-causes has nothing to do with the possibility of giving them a structural-functional explanation. Jarvie's own explanation of the cults is quite a good one, but no more than the ones he criticises does it explain why the cults arise in some places and times and not in others. When we distinguish between functions as causal effects and functions as purposes, and also distinguish functional relations as a special type of causal one, the temptation to argue, as Jarvie does, that cargo cults can have no functional explanation is removed. For the functionalists never wished to imply that the function of an institution like the Kula ring must be a social purpose. Of course, there are institutions which being deliberately devised for a purpose have a function in both senses of that word. But this has not been denied by anthropologists since the time when Malinowski first drew their attention to the causal sense of ' function '.

Despite—in part because of—some shortcomings, *The Revolution in Anthropology* ought to be widely read. It will arouse academic irritation and opposition wherever it goes, and this alone should make us welcome it. However, the author does take up with both enthusiasm and a flair for provocativeness many topics of considerable interest. His description of the cargo cults and the explanations offered of them is neatly done, his criticisms of these explanations is both pointed and stimulating; and his brisk asides on such questions as fieldwork, rationality, and fact worship will enliven a clutch of sombre seminars. Above all, the book is a pioneering effort which needs to be followed up by discussion of the various issues it raises in such lively fashion.

For the second edition the author will want to correct a number of unimportant misprints, change the sentence on p. 164 from ' If there are poor non-cultists then the theory that all cultists are poor is false ' to ' . . . that all poor people are cultists is false '. He may also wish to consult the *O.E.D.* entry for the word ' proof ' with respect to his footnote sentence on p. 39: ' Nothing but the propositions of logic and mathematics *are* even remotely capable of anything like proof '. And he may be happy to eliminate the words on p. xv which read: ' beginning to work forward until I could really pinpoint where I got stuck. This point was at the revolution in anthropology '.

Robert Brown

REVIEWS

Scientism and Values. Ed. Helmut Schoek and James W. Wiggins. The William Volker Fund Series in the Humane Studies.
Van Nostrand, Princeton, 1960. Pp. xvi+270. 49s.

Relativism and the Study of Man. Ed. Helmut Schoek and James W. Wiggins. The William Volker Fund Series in the Humane Studies.
Van Nostrand, Princeton, 1961. Pp. x+259. 49s.

THESE two companion volumes of symposium papers overlap in content and contributors, as well as editors. I wish to review them together because they are intended as coherent parts of a series (a third volume, *Psychiatry and Responsibility*, has already appeared). This series, sponsored by the William Volker Fund, might be described as anti-radical critiques of the social sciences. The intellectual mentors of the series are von Mises and von Hayek; the former being a noted right wing and *a priorist* economist (see Anne Martin's review in this *Journal*, 1962, **13**, pp. 65-70), the latter having coined the word ' scientism ' and investigated the history of the cult.

In his editorial introduction to *Scientism and Values* Schoek says that scientism is the misapplication of the methods of science to other studies. It is a misapplication because, among other things, scientism encourages the fruitless search for ' ethical neutrality ' (more often in practice a disguised progressivism), and its apparatus of statistics and quantitative techniques tends to blind social scientists to their objects of study—people. The main theme of the *Relativism* volume is to criticise misapplications of relativism, especially those intended to promote an omnitolerant liberalism. In both, then, the main impulse of the contributors is to trace political error (radicalism) back to methodological mistakes (scientism and relativism).

In all there are a dozen papers in each of these volumes and all I can do is to describe certain general features of the volumes and detail a few of the interesting papers.

Generally speaking the volumes, especially *Scientism*, are vitiated by the polemical tone of some of the papers, which are very short on argument, and not particularly open about their own commitments. For example one will find an author discussing the claims of another to be ' value free 'or to be a thoroughgoing relativist and carefully revealing the progressivism or hidden absolutism underlying these claims. But the point is not simply made: it is also intimated, but not argued, that social scientists should not be progressivist. For example one of the editors, James W. Wiggins in his' Fiduciary Responsibility and the Improbability Principle ', argues as follows. Sociologists' professional organisations have an institutionalised progressive tinge, e.g. they have tried to abandon the whole concept of race, or, after admitting racial differences still persist in using their scientific authority to campaign against the idea that the differences should make any difference. Even where there are correlations between racial intelligence differences social scientists turn their backs, and then lean over backwards to boost the minority. Social scientists should not act like this but be detached, like judges, seeking the truth, not making it prevail: this is their responsibility to the society which supports them. For myself, I could not see that Wiggins's argument told at all against those ' progressive ' anthropologists who admit there may be racial differences and who, at the same time, campaign against the idea that the differences should make any difference.

Of the papers in the *Scientism* volume much the easiest and most rewarding to read are ' Scientism in the Writing of History ' by Pieter Geyl, and ' The Psychopathology of Scientism ' by Ludwig von Bertalanffy. Geyl is very brief, simply suggesting that Hegel and Comte fathered the scientistic idea that there are laws of historical development. He suggests that the appeal of such laws lies in that they confirm the historian in his search for certainty and make him feel virtuous because he has seen the pattern. Geyl reaffirms his well-known counter view that there is ' no certainty, no finality ' in history; he believes, there is, however, a truth to be sought and about which there will always be disputes. Bertalanffy's is a well-constructed paper which devotes its first two parts to a précis of the scientistic doctrines he wants to criticise:

> Our knowledge of the laws of physics is excellent, and consequently our technological control of inanimate nature is almost without limit. Our knowledge of biological laws is not so far advanced, but it is sufficient to allow a large amount of biological technology in modern medicine and applied biology. Our knowledge of the laws of human behaviour and of society is still undeveloped. Consequently, human and sociological technology lag behind physical and biological technology. If we had a well-developed science of human behaviour and society and a corresponding technology, this would mean the way out of the personal, sociological, and political problems of our time (p. 207).

Bertalanffy argues that this attitude will destroy science and the free society. His argument is that it will lead to an increasing stress on technology, a dominance of ' group ' research, a distrust of personal responsibility, and a bureaucratisation of science which will kill it—by killing the abstract and theoretical, personal and idiosyncratic studies on which its growth depends. As to the free society:

> Conditioned-reflex methods, ' hidden persuaders ', ' brainwashing ', subliminal motivation, and allied techniques form the basis of psychological coercion and control unknown in previous history . . . Replacing old-fashioned rhetoric and appeal to limited numbers, modern psychological techniques have made this method scientific, all-inclusive, and nearly infallible (pp. 212-213). . . the basic defect [of scientism] can readily be summarized. It is the mistaken belief that science, scientific method, and technology with its achievements for human comfort cover the whole of human experience and fulfillment . . . scientism cannot provide a basis for the uniqueness of human individuality and values. In a reappraisal of the latter will be the clue to the future (pp. 215-216).

One may not be as pessimistic as Bertalanffy about brainwashing etc., to feel that his plea for more humanity is well said.

W. M. Werkmeister's ' Social Science and the Problem of Values ' says that we must distinguish the quest for objectivity (or truth) from that for ethical neutrality—belief in the latter only leads to smuggling in values. R. Strauss-Hupé in ' Social Science *Versus* the Obsession of " Scientism " ', says that the Greeks tried to study society objectively but that their efforts were swamped when the success of natural science gave birth to scientism and its bastards like the bogus objectivity of behaviourism.

Taking up the problem of objectivityW. T. Couch in ' Objectivity and Social Science ' emphasises that bias cannot be got rid of. We are all biased, is his thesis. This is partly because even our ability to see the external world without bias is suspect. He points out that many social scientists become relativists in the course of striving for objectivity or impartiality. But in becoming relativists they jettison their rationality and are forced to pretend that it is not the case that some societies are better than others. Were they to be consistent in this they would be in no position to preach for a rational social order as does Ruth Benedict.

In ' Science and the Studies of Man ' Eliseo Vivas takes up a different problem. His thesis is that the sciences of man are not exact sciences because there are within them schools of thought, irreconcilably opposed. There is, e.g. no Marxist physics (Vivas says). To the objection that pure science also has factions, not just on the frontiers of discovery, Vivas replies that these are of lesser degree and that they tend to be ' philosophic ': about the established facts there is agreement. Vivas's explanation of why the sciences of man are not exact is that only those aspects of our lives which can be quantified can be the objects of exact scientific study. Vivas believes that much that a pure scientist works on is public and precise, while man and his values are vague and not so public.

The co-editor, Helmut Schoek, in ' Knowledge: Used and Misused ' mentions interesting lacunae in sociological studies, like the behaviour of blood-donors, which is ignored by progressive sociologists who prefer to believe that social good can be done only by governments—it cannot be left to people themselves. He also is concerned to argue that *critics* of advertising and hidden persuasion believe in scientism and fear it, they too easily assume that people can be conditioned or coerced out of their ' nature ' (e.g. their dislike of ' mixed ' neighbourhoods).

Henry S. Kariel's ' Social Science as Autonomous Activity ' points out that current sociology concerns itself with functions and structures—it sees only what functions and fits. But man is part of nature and therefore biased: he can only control it, not be objective about it. His bias towards what fits, is measurable, and can be controlled, will lead to the crushing of the socially divergent, to an increase in the area of human behaviour which is studied and controlled. These tendencies are anti-democratic, and social engineering will replace choice. Whatever the good intentions behind it, scientism, in ignoring or attempting to ignore values, will only lead to an indifference to human uniqueness and freedom.

In other papers Richard Weaver attacks 'Concealed Rhetoric in Scientistic Sociology' where appeals are made to the authority of big names or fashionable ideas to plug something that would be better called social philosophy. Murray N. Rothbard in 'The Mantle of Science' advocates Von Mises *a priorism* and presents (pp. 174-176) an excellent critique of *Wertfreiheit.*

Certain common themes clearly emerge from the *Scientism* volumes, while other conflicting strains are to be seen. In particular there is a clash between those afraid of hidden persuaders (Kariel), and those who insist that they ought not to be overrated (von Bertalanffy and Schoek). There seems to be agreement on the point that social scientists pretend to be objective and are concealed progressives and radicals, that they use their scientific authority to promote radicalism (Weaver). I was a little disappointed that no author attempted to explain the fatal lure possessed by the scientistic cult. In my view it is the promise of results (salvation) either quickly (just copy science) or at least without too much effort (mechanical application of a method). It is an easy way to turn a vague, directionless, and perhaps disreputable (emotional, political, romantic, or even prurient) 'interest in society' into a respectable science.

The best essays in *Relativism and the Study of Man*, so far as I can see, are those by von Mises, Vivas, Leoni, and the two on linguistics (Pei, Weaver). Von Mises's 'Epistemological Relativism in the Social Sciences' is about praxeology, the study of human choices which, it is asserted, is about means of achieving given ends, not about the choice of ends. According to von Mises, Weber and Knies criticised praxeology because it had no historical dimension: it was universal rather than relative. However, von Mises counter-argues that even Weber's idea of 'rational action' involves judging means and ends, because he does not allow magic and religion to be legitimate *zweckrationales Handeln.* So Max Weber, who tried to free social science from value-bias only succeeded in adding in more value-judgments. Von Mises concludes that our privileged access into 'human action', our ability to understand and interpret it, is our *a priori* praxeological framework: our *a priori* knowledge consists in our being human.

Bruno Leoni's paper, 'Some Reflections on the "Relativistic" Meaning of *Wertfreiheit* in the Study of Man', points out that many of the arguments which have been directed against Weber's *Wertfreiheit* idea were either answered by him already or are borrowed from his own criticisms of his opponents. Weber's main aim in advocating *Wertfreiheit* was his moral opposition to *professorenprophetie* being served up as impersonal science.

Weber did not mean that social science stopped bothering about values, or that all values were equal; indeed he argued that moral choices are sometimes between incompatibles.

> In fact, his general programme for the social sciences might be better labelled as 'freedom from confusions' than as 'freedom from values'. . . . What he wanted . . . was to try to keep separate statements relating to empirical facts and statements relating to evaluations of those facts. To adversaries who told him that it is difficult to keep facts and evaluations separate, he replies, 'Yes, but let's try' (p. 167).

Eliseo Vivas, in 'Reiterations and Second Thoughts on Cultural Relativism' argues that the fact of cultural pluralism has misled many anthropologists into cultural relativism. He analyses the transition into the syllogism:

(1) A culture determines the values acknowledged by its members.

(2) The values determined by a culture are the values that the members of that culture ought to acknowledge.

(3) Therefore culture determines the values its members ought to acknowledge.

He is able to show that (1) is empirical and not conclusive; (2) he feels overlooks the difference between what is done and what is desired to be done, and he challenges the idea that the authority of a code can come from the mores. Culture, he says, is polysemic—when in Dobu do as the Dobuans do may or may not be problematic; but what *the* Romans or *the* Americans do is impossible to say. He also argues that cultural relativism leads to individual relativism or nihilism. Besides, 'all values are relative' if true, being a value, is false; and if true is at least one true nonrelative statement and others may be obtained by the same method that this one was; and even if we say it is 'true' in a second-order sense it is nonrelativistically true and by the method it was obtained we may get further nonrelativistic truths, even if they are second-order. Finally Vivas gives an excellent critique of Herskovits relativism.

The first paper on linguistics, 'Relativism in Linguistics' by Mario Pei, is an historical survey of linguistic relativism as featured in the theories of a universal grammar or basic language and in essentialist versus nominalist views of words. The tendencies culminate in Whorf

> who asserts that not only does the type of culture influence the language and its development, but that the converse is true; in other words, we are influenced in our mode of thinking and our behaviour by the type of language we habitually speak (a language whose verb has clear and precise tense distinctions, for instance, will induce in its speakers a keen consciousness of time values and punctuality, while a language in which the action is represented merely as occurring, without reference to time, will create in its speakers a sense of timelessness that will lead them to shrug their shoulders at time clocks and due dates) (p. 221).

Pei's conclusion is that the truth lies in between relativism and absolutism. Languages are stable as well as changing, they share characteristics as well as differ from each other. We must accept that all languages are equal, but not all are as developed, as elegant, etc.;

> relativism must be accepted and applied with caution, circumspection, and conservatism—in other words, with numerous and large grains of salt (p. 233).

' Relativism and the Use of Language ', by Richard M. Weaver opens up further the question of whether the fact that language changes should give comfort to the relativist. It raises the problem of the correspondence between language and reality; the author's view is

> that meaning makes things 'things'. It does not make things in their subsistence but it does make them in their discreteness for understanding. Extramental reality may itself be a nameless flow of causality, but when we apperceive it, we separate it into ' discretes ' such as ' house ', ' tree ', ' mountain '... To know a thing is not to arrive finally at some direct perception of a property, as Locke suggests, but to form some ideal construct of it, in which meaning and value are closely bound (pp. 240-241).

Our sense of the precise meaning of a word is a seminatural faculty—*contra* relativism. The author then criticises ' rhetorical substitution ', which is shifting the meaning of a word when it has become out of date while keeping its previous value-associations (e.g. ' liberalism ' used to mean personal liberty but now it means something like the welfare state); and ' rhetorical prevarication ' which is the conscious extension of a word in order to change our values like extending the category of ' sick people ', with all its overtones, to alcoholics and delinquents. This the author regards as scandalous. He concludes that language is a covenant, to be tampered with cautiously.

' Absolutes, Relativism, and the Scientific Psychology of Human Nature ', by Leonard Carmichael argues for the possibility that

> because of man's conservative and essentially unchanging inborn anatomical, physiological, and psychological characteristics, some of the legal, ethical, esthetic, and other values of mankind, once they are discovered, may also be considered to be fixed and unchanging (p. 8).

He believes in promoting those values which are ' advantageous for the human race '. Conway Zirkle in ' Human Evolution and Relativism ' says that scientists became relativists because they were humble about their ignorance. But this gives rise to the problem of how we distinguish our science from quackery, how do we know our approximations to truth are good approximations? We must base our work on assumptions we cannot prove but which we prefer because they work. Western culture is not a monolith but a collection of subcultures some viable and others not, those not viable eliminate themselves. The author seems to conclude with recommending a pragmatic attitude towards everything from euthanasia to foreign aid.

Helmut Schoek in 'Relativistic Absolutists and Public Policies', wonders if we are

> not justified in doubting the relevancy of ideas that negate, even . . . remotely, the continuation of society as a meaningful value? (p. 75).

Our free society, he argues, leads us to tolerate those who would subvert it by spying, or by 'social experiment': where can we draw the line? Perhaps at survival-value? *En passant* he argues that relativism has a self-fulfilling tendency: criminals come to believe they are no worse than normals, Ghanaians believe that but for an accident or two they might be giving foreign aid to the U.S.A. I would like to have seen Schoek arguing with Kariel, whose paper in the *Scientism* volume contended that bias towards what functions, what fits, what promotes continuation, has an anti-democratic, anti-libertarian tendency.

In 'Relativism and Social Control', John W. Tietz surveys the history of relativism and discusses the misplaced rise in stature it got from misinterpretations of Einstein. As a result of the spread of relativism to education and jurisprudence

> All sense of responsibility is taken from man. In this condition man loses his dignity and his sense of any responsibility. He becomes completely expendable to his environment, of which he becomes either a slave or a victim. Again, in the field of ethics and conduct, relativism leads to an oscillation among goals, an ambivalence in thought and feelings, or even worse, an ultimate confusion because ends are not clearly seen (p. 213).

As I hope can be seen from my attempts to summarise the various papers of these volumes the topics are of considerable interest to philosophers and methodologists, but the papers vary a great deal in quality. A number of the papers set themselves a problem and then try to solve it. Most, however, are polemics rather than genuine academic inquiries. That is to say their aim is to combat a mischief: scientism, relativism, liberalism, radicalism. Yet they all are thinly disguised as detached academic critiques. It is true that sociology still has a left-wing bias, and its historical origins in radicalism make this perfectly understandable. Few of the contributors seem much interested in why the social sciences are radical, they assume it to be a dreadful inexplicable affliction. It was Sidney Webb, I believe, who thought that just telling people the *facts* about society would make them radicals. Modern social science certainly grew out of this: it was the radicals who were critical of society and therefore marshalled facts as arguments for their reforms. Now that the radicals are so entrenched there is certainly a need for counterstudies by those more conservatively oriented. The radical studies problems he thinks need tackling. The conservative studies society to protect it from the rampages of the radicals. It is understandable that there should now be an anti-left revolt in the social sciences, just as in economics there have been various anti-right revolts. To me that best state of affairs would be when there are both right and left-wing social scientists, neither discriminating against the other, but carrying on a continuing debate about social problems, their solutions, and their remedies. Far from being value-free, I should have thought social science should be value-loaded, but explicit about it. The contributors to these volumes are regrettably not explicit enough about the political content of their critiques. The true enemy, it seems to me, is self-deception, such as the idea that social science *must* take a particular political stance, or the use of ' scientific ' authority to plug a political line as though it were an objective finding. In drawing attention to these serious faults these volumes are valuable.

I. C. JARVIE

The Structure of Scientific Revolutions. (*International Encyclopedia of Unified Science* : Vol. II, No. 2). By Thomas S. Kuhn.
Chicago University Press, 1962. Pp. xv+172. 22s. 6d.

THE purpose of this book is to reconsider and reconstruct the history of science in the light of the recognition, made in recent years by a few historians of science, that the actual development of science does not fit the old picture of science as a stockpile of facts and techniques growing by piecemeal accumulation. This is the basis of Kuhn's position. The contributions he offers are first the idea of a ' paradigm ', and its correlative ideas ' normal science ' and ' revolution '; second an analysis of how revolutions work, whereby a period of normal science under one paradigm gives way to another period under a different paradigm.

On reading this book, one's first impression is of enthusiasm and vitality. The author clearly feels himself to be opening up a new world of appreciation and understanding. In the face of such force and charm, it seems mean to question the lasting value of the work; but it must be said that many of its features are already well established (to take the first example—what decent historian of science, for many years now, has looked upon himself as a chronicler of piecemeal accumulation? pp. 1-2); and the author's enthusiasm leads him to over-state his novelties in a way that prejudices the appreciation of those things of value he has to say. In the following remarks I will try to present a sober view of the work—while very conscious of the fact that many other readers (who are less curmudgeonous than I) may well find the book very exciting. And I would wholeheartedly endorse its great value to beginning students—though I hope they will be warned that it gives a misleading picture of the modern history of the historiography of science. It will be good exercise for them to discuss the author's excesses.

The author's choice of the word ' paradigm ' to express one of his main ideas brings out his point of view. He defines a paradigm as a ' universally recognised scientific achievement that for a time provides model problems and solutions to a community of practitioners ' (p. x; cf. p. 10). This description would seem to apply to what others would call a ' basic theory '; but his point is that ' basic theories ' determine normal science *regulatively*, and he wants his technical term to mark this. Paradigms govern normal science. However, it did not seem to me that the normative overtones of the word ' paradigm ', as the author used it, were appropriate when he was describing historical developments. I would suggest, in fact, that if a reader wishes to bring out the real content of what Kuhn is saying, he may find it advantageous to try substituting ' basic theory ' for every occurrence of ' paradigm ' in the book. He will come across very few places where the sense suffers (many statements are made about legitimacy and rules, where the content is carried explicitly by an appropriate word); and a careful study of these will be more illuminating than the ubiquitous use of the odd word 'paradigm '. The author notes that ' the search for a body of rules competent to constitute a given normal research tradition becomes a source of continued and deep frustration ' (p. 44), for paradigms act ' without the intervention of discoverable rules ' (p. 46): but he is still concerned with their normative rather than their cognitive action.

Kuhn's view of ' normal science ' leads him to conclude that ' normal science does not aim at novelties of fact or theory. . . . [These are] produced inadvertently by a game played under one set of rules, their assimilation requires the elaboration of another set ' (p. 52). The collapse and new-creation of paradigms represents ' scientific revolution '. Changes are produced under the pressure of anomaly and crisis; and anomaly and crisis show up against the old paradigms, which at the same time suppress their recognition (Sect. VI-VIII, pp. 52-90). Here Kuhn brings out something of great importance to the understanding of change, and he illustrates his discussion with many well chosen historical examples. But can we accept his account of the forces which govern change? Who would change a theory unless there was something wrong with it? Can one recognise any result as odd unless one has some framework of expectation into which to fit it? The facts are, that inertia of mind, inability to work except within a fairly restrictive framework, inability to see the inadequacy of one's own well-established opinions, all these are general qualities of mankind. It is misleading to explain the source of these limitations by reference to shared paradigms. Kuhn, however, accepting this explanation, seeks to explain the regulative force of paradigms; and he finds this in the scientist's education. He finds that historical fact is distorted so that a cumulative and authoritative picture is presented, and he supposes that this tendency is probably functionally ingrained in the scientific ideology (p. 137). The result is an education which is ' narrow and rigid, . . . probably more so than any other except perhaps in orthodox theology ' (p. 165). It seems to me that the author's good ideas get rather submerged.

Kuhn's concern with the legitimating quality of paradigms, as he sees them, appears particularly plausible in the context of revolution. It does sometimes look as if there is a collapse, as if conventional rules were being questioned where there are no objective standards of truth. ' As in political revolutions, so in paradigm choice—there is no standard, higher than the assent of the relevant community ' (p. 93). Kuhn argues his case well (Sect. IX); but it seems to me that he does not distinguish sufficiently carefully between the impossibility of appealing outside all experience, and of appealing outside any (particular) experiences, and that he does not analyse very deeply the cognitive relations between competing theories. Perhaps as a consequence of this, Kuhn seeks to go quite a way towards saying that a revolution is the inauguration for the scientist of a new world, and not merely of a new description of *the* world (Sect. X). He supports this in two directions. First he brings forward the usual difficulties about perception: what does one see, and how does one describe it without theoretical commitment? Here he gives a number of good new examples, but those who have not been impressed by Hanson, Hesse, Feyerabend, and others who have taken these lines are not likely to be converted by Kuhn. The second line of argument refers to the way ' the facts ' change, supposedly due to a change of theory. He takes as his main example the effects of Dalton's atomic theory (pp. 129-34): the same measurement techniques used to determine the proportions of oxygen in the oxides of copper yielded different concrete results when these techniques were applied to nature under the old paradigm and the new. But what does this amount to? If a theory is true, more accurate measurements will produce a closer fit; but it is not the change of theory that produces the better fit. About a hundred years later, still more accurate measurements of weight ratios in compounds were made under a refined theory of atomic structure; but then no amount of ' beat[ing] nature into line ' (p. 134) helped—for the ' paradigm ' in question was false. Perhaps Kuhn should bring out more clearly the differences between determining what observations shall be made, and determining what the observations shall be.

Though it seems to the reviewer that the author has a view of science and its relation to nature heavily over-weighted in the direction of convention and rules, it must be admitted that not only the author but many readers would say that the reviewer was over-weighted in the direction of description and cognitive theory. The difference is seen in their attitudes towards method. Author and reviewer would agree that there are good and bad methods in science: but one would describe the good ones as ' legitimate' and justify them by appeal to rules and normative paradigms; while the other would describe the good ones as 'appropriate' and justify by appeal to the facts as revealed in basic theories which are cognitive. Kuhn has much of value to say against the latter view, Section XII 'The Resolution of Revolutions' being perhaps the best. But both would agree that neither approach has been worked out adequately by *anyone*.

In view of the wealth of well chosen historical and other examples, it is unfortunate that there is no index. Perhaps this will be remedied in the next edition.

H. V. STOPES-ROE

Scientific Explanation, Space and Time. Volume III, Minnesota Studies in the Philosophy of Science, edited by H. Feigl and G. Maxwell.
University of Minnesota Press, Minneapolis, 1962. Pp. xv + 628. £3 8s.

WRITING on the philosophy of science is now exposed to one of the chief evils of affluence—Parkinson's Expansion Effect. This latest volume from the Minnesota Center possesses, as its title suggests, a number of the features of incipient gigantism: forbidding size (and price) combined with a majestic span of topics which are amply elaborated. The contributions consist of two very long papers on space and on time by Grünbaum and Sellars respectively; two shorter ones on the analytic-synthetic distinction by Putnam and by Maxwell; two papers on theoretical entities and theoretical concepts by Maxwell and by Rozeboom; and a group of four essays on various aspects of scientific explanation. It is only this last group which will receive more than mention here.

In a two-part essay Hempel discusses some of the criticisms made of his version of deductive explanation and alters his previous account in certain respects. He goes on to contrast this covering-law model with statistical explanation, and while it is to this latter subject that he devotes most attention, his replies to criticism of his model are also of interest. He accepts the complaint that not all examples of covering-law arguments supply explanations. For example, given some theorems of physical geometry we can deduce the height (80 feet) of a vertical pole after learning that it 'subtends an angle of 45 degrees when viewed from the ground level at a distance of 80 feet'. But these premises do not provide a causal explanation of the height of the pole—tell us why it has the height it now has.

Surely what these premises are capable of explaining in certain contexts, is why the pole *must* be 80 feet high rather than some other height, that is in knowing such theorems as that an isosceles triangle has two equal sides, and knowing, also, that the theorems apply to the case of the pole, we know why it is that the pole must measure 80 feet in length—and know why it must.

Correspondingly, we can correctly predict its length while not being able to predict that a given agent or agency will cause the pole to have that length. True, our premises can neither explain nor predict the pole's height in the sense of supplying a causal story. But only the assumption that 'why' questions require causal answers could lead us to demand such causal conclusions from non-causal premises. The important point about the supposed symmetry between covering-law arguments and scientific explanations has been made many times: a scientific explanation of the universal type employs a covering-law argument to answer a particular sort of question in a certain setting. The mere presence of the argument does not ensure the presence of an explanation.

Hempel's treatment of statistical explanation lays emphasis upon three ways in which it differs from deductive explanation: (*a*) the former does, and the latter does not, raise the question how to take account of all available relevant evidence in establishing the degree of confirmation to be assigned to a hypothesis; (*b*) only inductive explanation sets us the problem of formulating rules or criteria for the rational acceptance of hypotheses that are strongly supported by evidence; (*c*) a deductive explanation will explain the conjunction of any hypotheses it explains separately; an inductive explanation need not. Concerning the first question, Hempel suggests that the difficulty of actually applying 'the requirement of total evidence', for example of showing, in any given case, that some of the total available evidence is inductively irrelevant to the conclusion, can be met by a substitute rule. This is: given the generalisation $p\ (G, F) = r$, use 'the statistical probability of G within the narrowest class for which the required statistical probability is provided by the total available evidence'. But Hempel goes on to point out that the rule does not work in all cases. Those with premises like '*Fb*' and '$p(G,F) = \cdot 9999$' and conclusion '*Gb*', may have included in their total evidence the statement Hb and information to the effect that 10,000 instances other-than *b* were *H* and non-*G*. The argument '*Fb* and $p(G, F) = \cdot 9999$ therefore *Gb*' meets the rule—since the total evidence does not give '$p(G,H)$'—without acceptably explaining or predicting '*Gb*'. This being so, Hempel seems to conclude that the requirement of total evidence is both indispensable and unsatisfactory.

The search for criteria of rational acceptability of hypotheses is treated by Hempel as a problem of maximising the estimated utility of accepting, rejecting, or suspending a given hypothesis with respect to 'the total body of scientific knowledge at a given time'. While he criticises the results of attempts to find a measure of this 'epistemic utility' in monetary terms or those of purely scientific gains and losses, he does not spend much time on the question of which factors such a utility could be based on, though he does mention some troublesome ones. Nor does he criticise, as many people would, the very quest for an 'epistemic utility'.

Feyerabend attacks, in his footnotes, Wittgenstein on language, Oxford-type philosophers interested in 'the language of savages or of idiots', confirmation theorists, and methodologically spineless historians. In the body of his text he attacks two views said to be held by Nagel: (1) the reduction of one science to another is (or should be) by logical derivation, and so is the explanation of one theory by another theory (2) 'the meanings of (observation) terms are invariant with respect to both reduction and explanation'. Of (1) Feyerabend tries to show, by means of such examples as the difference in the Galilean and Newtonian treatment of vertical acceleration, that an entailment relation is impossible—because the corresponding laws are logically incompatible. This situation, he argues, holds in general of sciences which are reducible and of theories which explain other theories. In both cases the actual relationship is that of substitution: the previous laws and concepts are replaced by more satisfactory ones, that is ones that better account for either the same or similar kinds of events. Feyerabend's argument is surely correct, but its outlines are obscured by his failure to treat separately, two different situations: that in which unsound theories cannot be entailed by sound theories because of logical incompatibility, and that in which a restricted theory is embraced but not entailed by a wider theory. Conflating these two situations, Feyerabend—because of his belief in the theory-laden character of observation terms—is led to speak, rather strangely, of a wider and a narrower theory dealing with the same phenomena. But if there is complete replacement of concepts, in what vocabulary are the 'same phenomena' to be described? Not surely, in some theory-laden terms which are different for the two theories?

Scriven attempts to revise the covering-law model of explanation. In doing so he comments on most of the major topics with which that model might be associated: laws, causation, determinism, reductionism, understanding. Amongst many dubious arguments on a variety of issues he produces one designed to show that scientific explanations need not incorporate law-statements. He thinks that just as we distinguish between a descriptive statement and our evidence for it, so we should distinguish between an explanation ('That bridge collapsed because of metal fatigue in its girders') and our justification for advancing it, for example, our information about metal fatigue in general, test results of the broken girders, and eye-witness stories of the collapse. An explanation does not have to contain its own justification. If it did—if, for example, a law statement connecting metal fatigue with girder collapse were required for a complete explanation—we should be led into an infinite regress of demands for justification, since the inclusion of the law is supposed to answer the question 'How can you be sure that metal fatigue is connected with the collapse of the bridge?'

Clearly, there is something wrong with this revision of the covering-law model. Scriven seems to agree that a scientific explanation is an argument.

But then in supplying statements of laws and initial conditions we are not *justifying* an existing argument, we are *constructing* one whose logical soundness and accuracy of premise can then be justified, if required. Scriven's error rests, of course, on his view that a law is not needed to permit an inference from one fact to another fact which it explains or predicts, i.e. that causal connections are not law-like. However, the examples he gives in support of his view are unhappily familiar: I know why the carpet is stained, since I knocked the ink bottle over, without knowing any of the laws involved. The counter reply, for example, 'How do you know that it was your hand that knocked over the bottle?', is taken up by Brodbeck in her discussion of laws as inference-tickets. She follows this debate through its usual ramifications into lawlike hypotheticals and conceptual analysis, and precedes it with a criticism of Scriven's claim about the asymmetry of prediction and explanation, and his claim that the explanatory use of statistical laws demands the relinquishing of the entailment relation. She strongly supports the covering-law model where Scriven attacks it. But his points, while made with characteristic sprightliness, will hardly convince the uncommitted.

The entire debate on explanation would have been more interesting to outsiders, though not perhaps to the writers, if Feyerabend's two theses had been subject to vigorous examination by both Brodbeck and Scriven.

R. Brown

Foresight and Understanding. By Stephen Toulmin.
Hutchinson, London, 1961. Pp. 115. 18s.

Stephen Toulmin examines two questions in this short well written book : What is scientific explanation? and What makes an explanation a good one? In the first chapter he argues against the thesis that the aim of science is prediction, concluding that no form of the thesis can give an adequate account of explanation. However the rigour of his criticism of the prediction theory squares ill with the vagueness of his own account of explanation. To explain is to 'make sense' of the observed facts, to provide principles in terms of which they 'hang together', to make what before appeared 'arbitrary' appear 'natural and rational'. This is done by means of 'Ideals of Natural Order'. In the following two chapters two pieces of history—of theories of motion and of theories of matter—are presented in the light of the Ideals of Natural Order which inspire them. These pieces of history are well done, bringing out the fundamental assumptions of the scientists under the title of 'Ideals'. How Toulmin intends these discussions to help our understanding of the nature of explanation is unclear.

What makes a theory a good one? Toulmin presents a reformulation. Theories excel which outlive other theories in the battle for survival : what gives a theory survival value? Just as there can be no criteria for a well adapted species—species adapted in one environment may fail in another—so there can be none for the excellence of a theory—for a theory which explains the facts we know today may be unable to adapt itself to new discoveries. Theories of confirmation are doomed to failure. But their opponents fare no better. The view that confirmation must fail because any set of observations confirm an infinite number of mutually incompatible hypotheses is irrelevant to scientific work. Far from the scientist having an infinite number of hypotheses to choose between 'it may be a stroke of genius for him to imagine even a single one'.

In the chapter entitled 'Forms and Styles of Theory' Toulmin bases an elaborate discussion on a passage from Kant's *Prolegomena*. The passage concerns the *a priori* nature of the gravitational inverse square law :

> We find a physical law of reciprocal attraction applicable to all material nature, the rule of which is that it decreases inversely as the square of the distance from each attracting point—that is, as the spherical surfaces increase over which this force spreads—which law seems to be necessarily inherent in the very nature of things, and hence is usually propounded as knowable *a priori*. Simple as the sources of this law are, merely resting upon the relation of spherical surfaces of different radii, its consequences are so valuable. . . .

This passage is capable, it seems, of two interpretations. I take it to present the following argument : The sum of the gravitational force due to a body over any spherical surface containing it is a constant—no matter how large the radius of the sphere. (It would be more precise to speak of the integral of the normal component of the gravitational force due to the body.) The area of a sphere increases in proportion to the square of its radius, so that this constant sum of force is spread thinner—attenuated more—the greater the radius of the sphere. Therefore the intensity of the force at any point decreases in proportion to the square of the radius of the sphere on which it lies. Kant then declares the conclusion of this argument—the inverse square law of gravitation—to have been deduced from geometrical facts alone : an observation which has no foundation whatsoever, since the constant sum of the gravitational force over any surface—which is Gauss's law—is certainly not a geometrical fact. But, whether or not Kant was right in supposing that he had given an *a priori* proof of the inverse square law, he had given a correct proof of it. Newton's law does follow from Gauss's law by means of the geometrical argument Kant employs. I believe that Kant's argument is perfectly correct physics—in fact a credit to him—but that it does not warrant his philosophical conclusion.

Toulmin, however, takes the passage quite differently, and the difference arises, I think, from his interpretation of the phrase 'as the spherical surfaces increase *over which this force is spread*'. Toulmin interprets this as 'as the spherical surfaces increase *through* which this force *spreads*', whilst I interpret it as ' as the spherical surfaces increase *over* which this force *is spread*'. Accordingly, where I see only a statement of Gauss's law, Toulmin concludes that Kant is suggesting an analogy between the gravitational attraction of a point mass and the illumination due to a point source of light, or the emission of an incompressible fluid from a point source : is suggesting in fact that gravitational force is emitted from a body and spreads out infinitely through a space which is completely transparent to it. This analogy, Toulmin continues, though as suggestive as many more successful analogies, turned out to have neither ' pragmatic value nor a theoretical basis ', partly because it implies that force is governed by a conservation law—whilst energy turned out to be so governed ; partly because the nature of the propagation of gravitational fields remained obscure. Recently, speculations have been made about the propagation of gravitational force which may put Kant's argument in a new light.

Now there is not merely an analogy but, in one respect, an identity between the gravitational force due to a body and the emission of an incompressible fluid from a source : Gauss's law holds both for the summation of force *over* a surface and for the summation of the movement of fluid *through* a surface. It is because of this that the total force over a surface is called the ' flux ' over that surface. But this resemblance between gravitational force and the propagation of a fluid is not a resemblance between the *propagation* of gravitational force and the propagation of a fluid. There is no reason, save an ambiguous phrase, for supposing Kant to be making any speculations about the propagation of gravitational force.

Toulmin's analogy between a theory and a biological species puts a due emphasis on the ideas which characterise a theory and which may survive the falsification of the hypotheses they are used to frame. It is to that extent an attractive analogy ; but the implication—which Toulmin perhaps intends—that to give a scientific explanation is to propose ideas which help us make sense of the facts, rather than to propose hypotheses in which we attempt to state their causes, is false.

John Watling

Wittgenstein und die moderne Philosophie. By Justus Hartnack. (Translated by Rosemarie Løgstrup from the Danish: *Wittgenstien og den moderne filisofi*, Gyldendal, Kopenhagen, 1960.) Kohlhammer Verlag Stuttgart, 1962, Pp. 148. DM 3.60

In a small one hundred and twenty six pages Professor Hartnack gives a highly intelligent, very informative, yet easily understandable account of Wittgenstein's main thoughts in the *Tractatus* and the *Investigations.* The book divides into four main parts: An analysis of the *Tractatus*; a short chapter on its influence on Logical Positivism; a discussion of some of the major ideas of the *Philosophical Investigations*; and a long chapter on some essays by present-day analytical philosophers in Oxford.

The discussion of the *Tractatus* is largely expository. In twenty pages Professor Hartnack presents a clear, but by no means oversimplified, treatment of Wittgenstein's early ideas, omitting only the formal logical arguments. He concentrates on the 'Picture Theory' of language—which he criticises—and examines Wittgenstein's notions of identity or similarity of structure and logical form, taking up Russell's point that logical form *can* be described (and not only shown). It can, Hartnack believes, be discussed in sentences which are neither true nor false—and therefore in Wittgenstein's sense not statements at all—but which are instructions or explanations, like the legends on maps and graphs. I found only one instance of possible misinterpretation of Wittgenstein's views, namely, when Hartnack criticises him for thinking that objects (Gegenstände) constitute the substance of the world (p. 24); but he makes this criticism only with hesitation.

In the Chapter about the influence of the *Tractatus* on Logical Positivism Hartnack sees agreement between Wittgenstein and the Vienna Circle in the belief that philosophy is not a system, but an activity consisting in the exposition and analysis of the *meaning* of statements; that Logic and Mathematics are tautologous, while empirical propositions are not true of necessity; and that the meaning of a statement is identical with its truth-conditions. Disagreement, he says, begins over the Verification Principle which was less dogmatically and less exclusively *the* criterion of meaning for Wittgenstein than for the Positivists. It increases over the possibility of there being more than one way of stating a (matter-of-) fact, which Wittgenstein denies, since for him a proposition is a picture of a fact and can therefore have only *one* logical structure, while Russell and the Positivists, especially Carnap, allow for a hierarchy of languages in which one sentence (say: S_2) can very well say something about the logical structure of another sentence (S_1). And finally there is complete disagreement over 'the Mystical' which for Wittgenstein can be shown and felt, but never described, while for the Positivists it is mere nonsense, since it cannot (logically) be thought or conceived. Hartnack says that if Wittgenstein really believed that 'the thought is the significant proposition' he should have seen that 'the Mystical' cannot even be thought.

A large part of the book is concerned with three important points in the *Philosophical Investigations*: (1) Wittgenstein's renunciation of this idea that there is and can be only one language, and his introduction of the notion of language-games which have only family resemblance to one another, together with the renunciation of his earlier belief that words denote objects and have meaning; (2) Wittgenstein's contention that correct *understanding* of propositions is the philosopher's main and proper concern—not the correction of loose language—and that phrases like 'I understand' are neither descriptions, nor reports, nor empirical sentences at all, but signals or exclamations like 'Hurrah!', and therefore neither true nor false; (3) the rejection of the possibility of a private language, using as an example the proposition 'I am in pain' as being, not the description or assertion of a feeling, but part of the (learned) behaviour of a person.

In the last Chapter, Hartnack discusses the main problems in four important articles or books by Oxford philosophers, who, he believes, have been strongly influenced by the later Wittgenstein. These are: *The Concept of Mind* by Professor Ryle, Strawson's article 'On Referring', H. L. A. Hart's 'Ascription of Responsibility and Rights', and J. O. Urmson 'On Grading'. In all of them, Hartnack says, there is agreement with the late Wittgenstein that many—perhaps most—philosophical problems arise from mistaking performative (ascriptive) expressions for descriptions, and that the division of sentences into 'descriptive' and 'meaningless' is not exhaustive. This last chapter does not really do justice to the enormous influence that the teaching and writings of the late Wittgenstein had on English philosophy, since it takes up only one point in each of the essays and discusses it rather briefly for its own merit. Possible Hartnack wished to give merely the most poignant examples of post-Wittgensteinian philosophy in Oxford. But in the relatively short treatment that was possible in this book he does not seem to have rendered a good service to either of the authors, although his discussion is sympathetic throughout. He does not draw any conclusions at the end of his book, nor is there a summary.

There is, however, an excellent appendix of twenty pages, which contains all the references and quotations from Wittgenstein, and some of Hartnack's own best criticisms. On the whole, Professor Hartnack's book can be warmly recommended as an introduction for readers, especially students, who are not yet acquainted with Wittgenstein's works—which is still the case for most Continental students of Philosophy (and many of their professors). The translation from the Danish into German is good and reads easily, and the modest price of the book (about 6s.) is a further recommendation. The professional British philosopher, however, will hardly find anything new or startling in it.

EVA CASSIRER

The Philosophical Impact of Contemporary Physics. By Milič Čapek.
D. Van Nostrand Co. Ltd., 1961. London and Princeton. Pp. xviii+414. 30s.

THIS is an interesting, scholarly, and well written book on the history of key physical ideas since classical Antiquity up to the late twenties. It deals with the concepts of space, time, matter, and motion both in classical and in contemporary physics, and with problems such as whether space might be constructed out of time sequences, and whether novelty involves irreversibility. The author traces the evolution of such questions and ideas and discusses their intimate relation to ontological problems, such as the problem of chance, and to epistemological questions, such as the relevance of phenomena to physical theory. His method of exposition, reminiscent of Meyerson's and Cassirer's though less digressing, puts the reader in contact with the original thought of hundreds of scientists and philosophers in the past. If only for this reason the book should be useful, particularly in countries with little sense of history.

Unfortunately, this book cannot be recommended to the nonphysicist: it has too many important mistakes. One is the lack of understanding of the revolution introduced in physics, and even in the world views warranted by physics, by the nineteenth-century field theories: the very concept of matter was essentially enriched when it was realised that not all the stuff the world is made of is corpuscular. This is not an isolable blemish, because the author claims that the most significant feature of classical physics is its pictorial character—a statement refuted by the mere existence of field theories. Another important mistake is the interpretation of the relational and causal theories of time as denying the reality of time for the benefit of an alleged spatialisation of physics: all such theories do deny is the independent existence of time. This, too, is important because it leads the author to an unnecessary crusade for the rescuing of time. The interpretation of '$E = mc^2$' as a universal law holding in all physical domains is mistaken as well, but in this case the author keeps good company: many physicists forget that the above formula is a theorem derived in relativistic mechanics, i.e. in a theory of particles.

Much more serious mistakes occur in the author's treatment of quantum mechanics. For example, his contention that Heisenberg's uncertainty relation for energy and time conflicts with the law of the conservation of energy (p. 322), and even that 'the concept of *quantity of any kind* loses its adequacy on the subatomic level' (p. 325). In this, as in other related matters, the author has been misled by some early vulgarisers of the quantum theory. For example, on p. 291 he adopts F. A. Lindemann's preposterous statement that in Bohr's quantum theory 'the conjugate coordinate to the square of the charge is the reciprocal velocity'. (The author holds a Ph.D. degree in physics, but it is obvious that his knowledge of contemporary physics is second hand.)

This brings us to a possible source of many of the book's blemishes: Čapek's reliance on elementary textbooks and vulgarisation articles. Such works tend to emphasise certain spectacular (counterintuitive) theorems of accepted theories and certain off-the-track speculations (such as the quantisation of space and time) which occupy much less room in the physicists' brains than in Čapek's book. As a consequence, individual results—such as Heisenberg's uncertainty relations—are discussed out of their theoretical contexts, and the strangest interpretations are proposed. The same holds for problems such as whether time is or is not an independent variable: how could one discuss this question but in the context of an axiomatic physical theory? It is only in such a context that the problem of the mutual independence of any given set of concepts can be solved. Isolated results, however important, can only be understood and evaluated by recalling the basic assumptions from which they derive.

Dissatisfied with both the classical and the contemporary views of the physical universe, Čapek wishes to find 'new ways of understanding' which be neither 'pictorial' nor 'panmathematical'. He proposes to search for them by exploring our own mind. The 'introspective model of physics' he favours himself is the 'imageless dynamical patterns' provided by music and which he states (without proof) are structurally similar to those disclosed by the new physics. Bergson's and Whitehead's dynamicism, and perhaps a bit of Kepler's neo-pythagoreanism, are thus revived : the world is a pure process, comparable to a musical piece; the stuff it is made of are events rather than either particles or fields. (This was seriously maintained by Whitehead, who had in mind the kinematics of special relativity. But how are we to 'understand', in terms of such matterless musicalities, events like the scattering of electrons by nuclei?)

But the reader is not bothered with these curiosities until p. 369: what comes before makes instructive and agreeable reading. The present reviewer wishes that a new edition, expurgated of all the important mistakes, could be published, because the author's aim—to study the deep philosophical impact of contemporary physics—is a serious one that has so far not been attained by anyone else.

Mario Bunge

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ARTICLES	
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EXPLORATION	
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DISCUSSION	
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BOOKS RECEIVED	
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VOLUME XV No. 59 NOVEMBER 1964

CONTENTS

ARTICLES

CONFIRMATION WITHOUT PARADOXES 177
by William M. Baumer

ON THE CONFIRMATION PARADOX 196
by P. R. Wilson

CONFIRMATION BY OBSERVATION AND THE PARADOX OF THE RAVENS 200
by Judith Schoenberg

A NEW APPROACH TO THE THEORY OF FUNDAMENTAL PROCESSES 213
by Mendel Sachs

REVIEWS

Scientific Change. Edited by A. C. Crombie 244
reviewed by P. K. Feyerabend

The Perception of Causality. By A. Michotte 254
reviewed by W. Mays

Understanding Physics Today. By W. H. Watson 259
reviewed by Ted Bastin

Philosophical Reasoning. By John Passmore 264
reviewed by I. C. Jarvie

Philosophy, Science and the Sociology of Knowledge. By I. L. Horowitz 266
reviewed by I. C. Jarvie

CONTENTS—(*contd.*)

Mathematical Logic and the Foundations of Mathematics. By G. T. Kneebone 268
reviewed by J. C. Shepherdson

The Language of Science and the Language of Literature. 1700–1740. By Donald Davie 270
reviewed by Herbert Dingle

RECENT PUBLICATIONS ON THE PHILOSOPHY OF SCIENCE 272

ANNOUNCEMENT 280

ERRATA 280

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CONFIRMATION WITHOUT PARADOXES*

William H. Baumer

The paradoxes of confirmation constitute one of those intrinsically minuscule issues that have consequences disproportionate to their size and are as a result the centre of much swirling controversy. Because their presence in a confirmation theory suggests that the theory is inapplicable or inconsistent, proponents of confirmation have sought to remove them while opponents have attempted to exploit them. This paper is primarily a presentation of a solution of these paradoxes developed on the basis of G. H. von Wright's confirmation theory.[1] Suffering from the misfortune of having been criticised in advance, it cannot be set out without reference to various other discussions of the paradoxes, including J. Hosiasson-Lindenbaum's attempted solution of them, J. W. N. Watkins's suggestion that attempted unsuccessful falsification does not encounter them, H. G. Alexander's explanation of them, and J. L. Mackie's attempt to resolve them.[2]

For those familiar with these paradoxes, repetition of them is tiresome. Nonetheless, I wish to present them once more in order to use

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* Received 18. iv. 63

[1] The author wishes to thank Professor J. L. Mackie, who read an earlier draft of this paper, for some most helpful criticisms.

[2] Cf. H. G. Alexander, 'The Paradoxes of Confirmation', this *Journal*, 1959, **9**, 227-233, and 'The Paradoxes of Confirmation—A reply to Dr Agassi', same *Journal*, 1960, **10**, 229-234, cited below as Alexander-1 and Alexander-2 respectively. J. Hosiasson-Lindenbaum, 'On Confirmation', *Journal of Symbolic Logic*, 1940, **5**, 133-148, and 'Induction et Analogie', *Mind*, 1941, **50**, 351-365, cited hereafter as J. H.-L.-1 and J. H.-L.-2 respectively. J. L. Mackie, 'The Paradox of Confirmation', this *Journal*, 1963, **13**, 265-77. J. W. N. Watkins, 'Confirmation without Background Knowledge', this *Journal*, 1960, **10**, 318-320, and 'Between Analytic and Empirical', *Philosophy*, 1957, **32**, 112-130, and 'A Rejoinder to Professor Hempel's Reply', same *Journal*, 1958, **33**, 349-355, cited hereafter as Watkins-1, Watkins-2, and Watkins-3 respectively. G. H. von Wright, *The Logical Problem of Induction*, 2nd revised edition, Oxford, 1957, n.b. Chapter vi, cited hereafter as von Wright, *Problem*.

the hypotheses, instances, and enumerated paradoxes as references in the remainder of this paper. Take a simple hypothesis and its contrapositive:

$$(x) . Fx \supset Gx \qquad 1.$$

$$(x) . \sim Gx \supset \sim Fx \qquad 2$$

which have, speaking roughly, four different instances:

$$FGa, \sim FGb, F \sim Gc, \sim F \sim Gd \qquad 3$$

that will be called *a*, *b*, *c*, and *d* instances respectively. According to J. Nicod, only *a* instances confirm 1; only *d*'s confirm 2.[1] This violates the equivalence criterion since the same instance then confirms one but not the other of two equivalent hypotheses. Call this the first paradox. It can be removed by defining a confirming instance as any non-disconfirming one.[2] But then *b* instances confirm 1 or 2, contrary to our intuitive expectations; call this the second paradox. It also follows that *d* instances confirm 1 while *a* instances confirm 2, and this violation of our intuitive views is to be called the third paradox.

What I propose to show is that a confirmation procedure which includes an eliminative feature, so that each confirming instance must eliminate some previously possible necessary or sufficient condition of a given property, removes the paradoxes. In particular, I shall show that such a procedure has the following results. First, the requirements of eliminative confirmation are such that *a* instances can be eliminative confirmations of 1 if and only if they are also such confirmations of 2; *d* instances have precisely the same status. Second, the requirements of eliminative confirmation are such that *b* instances are eliminative for neither 1 nor 2 and hence are never eliminative confirmations at all. They are, of course, important as falsifications of the converse of 1 or 2. Third, the requirements of eliminative confirmation are such that even where *a* or *d* instances do eliminatively confirm, there is an upper limit to the number of such confirming instances; those that can qualify as eliminative must have or lack, as the case may be, certain particular properties. Consequently, the mere fact that, e.g. there are indefinitely many *d* instances for a given hypothesis does not mean that it is well-confirmed since this does not mean that it is eliminatively confirmed at all.

---

[1] J. Nicod, *Le Problème logique de l'induction*, Paris, 1961, p. 23

[2] This is done by von Wright, *Problem*, p. 123, and is essentially the result of C. G. Hempel's theory in his 'Purely Syntactical Definition of Confirmation', *Journal of Symbolic Logic*, 1943, 8, n.b. pp. 130, 142; cf. his 'Studies in the Logic of Confirmation', *Mind*, 1945, 54, 109; these are cited below as Hempel-1 and Hempel-2 respectively.

The solution here advocated turns on a reinterpretation of von Wright's 'Principal Theorem of Confirmation':

$$P(g, e) > 0 . P(i_{n+1}, e . I_n) < 1 : \supset . P(g, e . I_{n+1}) > P(g, e . I_n) \qquad 4$$

While this interpretation is patterned after von Wright's, it differs from his at crucial points.[1] The central element is the taking of all the P's in 4 as ratios of the numbers of members of sets of properties specified by the various pieces of information included in 4. Consequently, g is 1 or 2 where these are taken as stating necessary or sufficient conditions of a given property. The initial evidence e is the specification of a set of properties A' or A''; which one of these two is chosen is, roughly, reciprocally dependent upon whether g is taken as a statement of necessary or of sufficient conditions. Taking A' first, let this be a finite set of properties such that all its members, G included, have been observed present with F in some instance, absent with F in another, and absent in F's presence in none. Then let B' be a subset composed of all the members of A', including G, that have been present with F in all the first n confirming instances; in the first instance, $A' = B'$. The information about B' constitutes I_n, that about $A' . B'$, $e . I_n$. Similarly, let C' be a subset of B' composed of all and only those members of B', G included, which are present with F in the new instance; this information constitutes i_{n+1}. The interpretation of the symbols in the consequent of 4 is determined by this, but will not be given here, since this is unimportant. The ratio of the number of conditioning properties (here of F in 1, $\sim F$ in 2) to $Nc(A')$ yields a positive value for the first P in 4. Plainly the second P can have a value less than one if and only if C' is a proper subset of B', i.e. if and only if some possibly conditioning property is eliminated by being absent in F's presence in the new instance.

The specification of the alternative content of e, I_n, and i_{n+1} parallels the above. Let A'' be a finite set of properties, F included, such that all its members have been observed present with G in some instance, absent with G in another, and present in G's absence in none. Let B'' be a subset of A'' containing all and only those members of A'', F included, that have been absent with G in all the first n confirming instances. Let C'' be a subset of B'' containing all and only those members, including F, of B'' that are absent with G in the new instance. Thus $e \,.\, I_n$ is the information about $A'' \,.\, B''$, i_{n+1} that about C''. Again the first P in 4 has some positive value when it is the ratio of the number of conditioning properties (here of G in 1, $\sim G$ in 2) to $Nc(A'')$. Again the second P can have a value less than one if and only if C'' is a proper subset of B'', which it is if and only if the new instance eliminates some previously possible conditioning property of G.

---

[1] Von Wright, *Problem*, p. 119, where the theorem is not entirely symbolised. For similar confirmation formulae, cf. J. H.-L.-1, pp. 133-134, and -2, p. 356; Nicod, op. cit. p. 66. Von Wright's interpretation of it is found in *Problem*, pp. 127-130.

The elimination requirement built into this interpretation of 4 directly and immediately removes the second paradox. Whether we begin with the set A' and read 1 (2) as a statement of a necessary (sufficient) condition, or begin with the set A'' and read 1 (2) as a statement of a sufficient (necessary) condition, no *b* instance can add to the probability of the hypothesis. This is so since no such instance meets the elimination requirements; if C' is to be a proper subset of B', F must be present (but is not); if C'' is to be a proper subset of B'', G must be absent (but is not).[1]

This elimination requirement is also relevant to removal of the first and third paradoxes, but in neither case is this as straightforward as was removal of the second. Take the first one first. If we set out to confirm 1 as a necessary (sufficient) condition statement, we thereby must read 2 as a sufficient (necessary) condition statement. Now, eliminative confirmation of a necessary condition statement requires that the conditioned property be present in every eliminative instance, while such confirmation of a sufficient condition statement requires that the conditioned property be absent. It also requires that suspected conditioning properties must be absent in the presence of the necessarily conditioned one, but present in the absence of the sufficiently conditioned one, to be eliminated. This comes down to no more and no less than: the requirements for eliminative confirmation of 1 as a necessary condition statement are the same as those for such confirmation of 2 as a sufficient condition statement, and conversely. In short if an *a* instance confirms 1, it confirms 2 ; if a *d* instance confirms 1, it confirms 2, and the converses of these two conditionals also hold. This removes the first paradox.

At this point an acute observer might detect a bit of an oddity, and ask, rhetorically, whether or not 1 as a necessary condition statement is equivalent to 1 as a sufficient condition statement. He then might go on to ask why no new paradox is engendered by the fact that eliminative confirmation, by use of the above machinery, of 1 read the former way apparently does not constitute confirmation of 1 read the latter way. This holds since, if we take 1 as a necessary condition statement, the obviously suitable initial set of properties is A', and only *a* instances can be eliminative with regard to this set. On the other hand, if we take 1 as a sufficient condition statement, the obviously suitable initial set is A'', and only *d* instances can be eliminative with regard to this set. It is at this point that the earlier remark (p. 179) about the reciprocal dependence of the initial evidence and our way of reading the hypothesis in question becomes relevant. Suppose our initial evidence is a specification of the set A', so that it is easiest to take 1 as a necessary condition statement; we could still take it as a sufficient condition statement. *If we choose the latter*, however, then since the information about A' is the initial evidence, we are not as it were searching for the sufficient condition of G; rather we are looking for that property *of which* F is a sufficient condition. But this is all one with looking for a necessary condition of F. The same sort of thing holds with regard to 2, and also holds if we have information about A'' as our initial evidence but insist on reading 1 as a necessary condition statement. Finally, it is worth noting that confirmation of 1 relative to A' and some instances is confirmation of 1 relative to different evidence than is confirmation relative to A'' and some instances. There is *no* problem in having the same (or equivalent) hypotheses having different degrees of confirmation relative to various bodies of evidence. (One further consequence of this: the choice of whether to use as our initial evidence information about A' or about A'' can be made at least in part on the basis of which sort of eliminative instances we expect will be easier to get: those in which F is present, or those in which G is absent.)

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[1] Von Wright states this point partially in his *Problem*, p. 131, but failsto realise no more need be said; cf. ibid. pp. 123-127, and his *Treatise on Induction and Probability*, London. 1951, pp. 254-256.

Removal of the third paradox is not accomplished by direct appeal to the elimination requirement in the above interpretation; indeed, it may at first sight seem that this procedure does not remove this paradox at all, for *a* instances can confirm 2 and *d* instances 1. But if we ask why the third paradox is one, we shall be on the way to removing it. The difficulty here is usually limited to confirmation of 1-like hypotheses by *d* instances on the grounds that there are many such instances, an overwhelming number, in fact, for any such hypothesis. This, it is then supposed, permits overwhelming confirmation of such an hypothesis, and may do so for one for which intuitively we should wish to claim no evidence at all. Certainly J. W. N. Watkins, a prime advocate of this criticism, is right in holding that any confirmation theory which permits such probabilification of an hypothesis to go on is thereby shown inadmissible.[1] But the theory here developed does not permit, much less require, such a procedure. First, it is not here the number, but instead the eliminative effect of *d* instances that is essential, given that the initial evidence is about A''. Second, since only eliminative *d* instances are then genuinely confirming, and there can at most be as many such instances as there are members of A'' less one, and the membership of A'' is finite, the number of genuinely confirming *d* instances is strictly limited. Finally, the properties a *d* instance must have, not lack, at any given point in the confirmation procedure are determined by the membership of B'' at that point; not just any old *d* instances will confirm here. Parallel considerations apply to the set A' and *a* instances, thus preventing bogus confirmation of 1 or 2 by collecting a large group of *a* cases. These considerations remove the third paradox.

Certain other elements of this interpretation of 4 need to be noted before we turn to alternative approaches to the paradoxes. First, the requirement that the set of the properties whose specification is the initial evidence be finite in size is not there merely to help remove the third paradox. This is also essential to satisfactory interpretation of the formula, since an infinite set here would make it impossible to speak of a ratio of conditioning properties to the number of members of this set. There is, additionally, something more than slightly odd about the claim that the generalisation is more probable after some members of such a set have been eliminated. Second, the demand that we have observed instances of the presence and of the absence, along with the presence and absence respectively of the conditioned property, of the properties whose specification is the initial evidence serves several purposes. It provides a non-arbitrary way of determining the membership of A' or A'', and at least tends to rule out confirmation of 'metaphysical' hypotheses. It also serves to prevent a difficulty many another confirmation theory invites.

This difficulty is best presented by illustration. Suppose we take: all Venusians have three and only three eyes, and seek to confirm it. If we can use *d* instances, and need no *a* instances at all (contrary to the requirement under discussion) to confirm this, we can do so either by enumeration or elimination. But we can similarly confirm, say, all Venusians have four and only four eyes; from all this it follows that there are no Venusians. Establishment of such a conclusion by reference to, say, the present population of the United States and with no examination of Venus just will not do. Nor will it do to assume, *pace* D. F. Pears, that there are Venusians, for this would simply leave us with two 'well-established' hypotheses and no basis for deciding which one, if either, is true.[1] In short, unless we know, not assume, that the classes mentioned in the hypothesis and its contrapositive have members, we can set out to 'confirm' hypotheses which are vacuously true or copiously true or for which we should claim to have no evidence at all.[2]

---

[1] Watkins-2, pp. 118-119; -3, p. 351

In sum, then, this procedure removes the first paradox by having *a* or *d* instances confirm both 1 and 2 if they confirm either. It prevents *b* instances from confirming either, thus removing the second paradox. It strictly limits the *d* instances, or the *a* ones, that confirm either, thus rendering the third paradox innocuous. Finally, it does all this without permitting confirmation of hypotheses for which we should not claim to have evidence at all.

For those who are familiar with other discussions of the paradoxes, perhaps the most surprising element in the above solution is the conclusion that *d* instances alone can provide confirmation on the basis of initial evidence including information about A'', and the implicit claim that they can then provide very good confirmation of 1. This is surprising because it is contrary to the usual view of such instances: that they are almost always relatively inefficient confirmations. The most thoroughly developed attempt to solve the third paradox on this basis is that by J. Hosiasson-Lindenbaum.[3] The fact that her solution runs counter to the one here presented is of particular interest because she develops it on the basis of a formal probability calculus which can include, as a theorem, formula 4 above, and, as axioms or theorems, all of von Wright's axioms and theorems; the converse also holds. While her solution does not make use of 4, the derivation of contrary results from the applications of equivalent confirmation calculi (for my purposes, they can and will be treated as the same, and are called hereafter 'the confirmation calculus') suggests that we may have excluded a set of paradoxes only at the cost of including a contradiction somewhere. This is, I now wish to show, not so; further, the procedure used by Hosiasson-Lindenbaum is less than satisfactory.

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[1] D. F. Pears, 'Hypotheticals', *Analysis*, 1949-50, **10**, 52

[2] The quotes in this paragraph are mine, used to indicate an odd sense of the enclosed expressions.

[3] Cf. J. H.-L.-1, *passim*

Hosiasson-Lindenbaum begins by stating four axioms which are interpreted in terms of the degree of confirmation afforded to a statement by some evidence. Then she informally derives some theorems, and the relevant one for resolution of the third paradox is that the less probable an instance is prior to being observed, the more its observation adds to the degree of confirmation of the hypothesis in question. Also required for the solution of this paradox are certain assumptions about the relative sizes of the classes involved (5, 6), and consequences derived from these (7):

$$Nc(F) = m;\ \ Nc(\sim G) = n;\ \ n > m \qquad 5$$

$$h_i = .\ Nc(FG) = i \quad (\text{where } i = 1, 2, 3, \ldots, m) \qquad 6$$

$$P(Gx, Fx\ .\ h_i\ .\ e) < P(\sim Fx, \sim Gx.\ \mathrm{h}_i\ e)$$

$$(\text{i.e.: } i/m < (n - m + i)/n) \qquad 7$$

On the basis of theorem, assumptions, and consequences, Hosiasson-Lindenbaum concludes that, given that there are more $\sim$ *G*'s than there are *F*'s, *a* instances provide stronger confirmation than do *d* ones. This holds since, according to 7, *a* instances are less likely prior to observation than are *d* ones ; moreover, the difference in strength of confirmation increases as the difference between *n* and *m* does. The third paradox is held removed by this since this shows that in most situations *d* instances are not worth examining; *a* ones are.[1]

One noteworthy feature of this solution is that it rests on the use of statistical probabilities, i.e. use of the ratios of the numbers of members of the several classes involved. Consequently, anyone impressed with the importance of the distinctions among various sorts of probabilities may react to Hosiasson-Lindenbaum's procedure with the comment that it rests on a confusion. It may be said that degree of confirmation is not statistical frequency, and that the supposition that one can proceed from evidence whose probability is determined in the latter sense of ' probability ' to the support of an hypothesis by that evidence in the former sense is simply a mistake. It may be due to confusion of:

$$\mathrm{P}((x)\ .\ Fx \supset Gx, e) = 1 \qquad 8$$

which includes 1, with the different, though entailed by 1:

$$(x) : Fx \supset .\ \mathrm{P}(Gx) = 1 \qquad 9$$

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[1] J. H.-L.-1, p. 134, n.b. note 3, pp. 137-138, 140

At any rate it seems that some such misidentification has been committed by Hosiasson-Lindenbaum here. Moreover, this critic may go on, while the paradoxes do arise with regard to 8, they are simply irrelevant to 9. Therefore, any solution of the paradoxes resting on such statistical bases is in fact no solution but an irrelevancy.[1]

The only way to show such a criticism mistaken is to provide a satisfactory transition from statistical frequency of the evidence instances to degree of confirmation of the hypothesis. An initially plausible way of doing this is that of taking the evidence instances as forming a restricted universe. Then, the statistical report of the proportion of instances of that universe that satisfy (i.e. do not disconfirm) the hypothesis can be taken as the degree of confirmation provided by that evidence for that hypothesis. This has various recommendations; e.g. within a restricted universe 8 and 9 may be identifiable. It also permits rebuttal of Hempel's criticism that Hosiasson-Lindenbaum's approach is not satisfactory in many situations since we often have no way of knowing that the assumptions she makes hold.[2] In a restricted situation, this information is obtainable; we can determine that the instances meet the conditions stated in 5, whether the instances be collected at random or not. Most importantly, of course, it yields a uniform interpretation of the calculus, since the probability of an instance can here be taken as the degree of confirmation afforded the description of that instance by the evidence in hand. Finally, it is interesting to note that Hosiasson-Lindenbaum has also claimed that a formula roughly identifiable with 4 can be derived from her axioms, and this can be used to determine the added probability the hypothesis receives by each observation of a non-disconfirming instance.[3]

There are three reasons for presenting this rather lengthy summary and rehabilitating amendment of Hosiasson-Lindenbaum's approach. The first is that the presentation thus suffices to show that though the same probability calculus is used, the interpretation given to the formulae thereof is quite different from that used in the opening part of this paper. Consequently, no suggestion of inconsistency can legitimately be drawn from any differences in the confirmation values these views assign to *d* instances. The second is to give the view a fair chance, since I wish to argue that it is simply not an adequate answer to either the third or the second confirmation paradox. It meets the first; as

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[1] This problem was suggested to me by Professor J. R. Weinberg of the University of Wisconsin.

[2] Hempel-2, p. 21, note 2

[3] J. H.-L.-1, pp. 133-134; -2, p. 356

Hosiasson-Lindenbaum observes, violation of the equivalence criterion is in fact violation of the axioms of the calculus.[1] This, incidentally, holds for any confirmation theory which includes both the general multiplication formula for probabilities and formula 11 below, since the conjunction of these entails the equivalence criterion. Thus, e.g. both von Wright's and Nicod's views have this as a logical consequence.[2] Partly as a result of this, though, Hosiasson-Lindenbaum's view does yield a confirmatory value for *d* and for *b* instances. This being so, it remains possible on this view to confirm an hypothesis, and confirm it highly, by appeal to *d*, or to *b*, instances, and the fact that many such may be needed is no problem since any such instances will do. This is not altered by including the demand that the initial evidence required in applying Hosiasson-Lindenbaum's version of 4 include specification of at least one *a* instance and one *d* one. Such a demand only prevents the derivation of such conclusions as ' there are no Venusians '; it still remains possible to confirm the hypothesis that all cows are black by finding one black Angus and then counting snowflakes, swans, and ravens. This alone is adequate grounds for rejecting this whole application of the probability calculus to confirmation.

But there is yet another and ultimately a more serious difficulty in this amended version of Hosiasson-Lindenbaum's view; this is hidden in the formulation of 4. In von Wright's proof of 4 it is held that the generalisation entails its instances and it is proved as a Lemma that the generalisation is at most as probable as is any of its logical consequences on the same evidence. Hosiasson-Lindenbaum, in arriving at her version of 4, takes the evidence in hand plus the hypothesis as entailing the confirming instance in question.[3] As a result of these essential assumptions, the condition in 4 that $P(g, e) > 0$ means that 4 can no longer be applied once the evidence or instances includes a *c* (falsifying) instance. In a way, this is all to the good, since the probability calculus also includes[4]:

$$P(s, e) + P(\sim s, e) = 1 \qquad 10$$

$$e \supset s \,.\, \supset . \, P(s, e) = 1 \qquad 11$$

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[1] J. H.-L.-1, p. 138, note 7. The equivalence criterion is her axiom iv, but this is redundant since her axioms i and iii entail it; see ibid. p. 133.

[2] Cf. von Wright, *Problem*, p. 93; Nicod, op. cit. pp. 65-66

[3] Von Wright, *Problem*, pp. 118-119; J. H.-L.-2, pp. 355-356

[4] Formula 10 is von Wright's axiom 1, 11 his theorem 2, in *Problem*, p. 93. For Hosiasson-Lindenbaum, 10 is easily derived from axioms i and ii, while 11 is axiom i; H.-L.-1, p. 133; -2, p. 353.

Because a falsifying instance yields a probability of nil for its related hypothesis, the conditions set in 4's antecedent must be unsatisfiable once such an instance is found. Otherwise, we shall contradict formula 10 by having evidence partially confirming but totally disconfirming the same hypothesis. But, the consequences of this for the attempt to base degree of confirmation on the statistical report of the proportion of instances in a restricted universe that satisfy (i.e. do not disconfirm) the hypothesis in question are not admissible. First, once the evidence universe has been completely examined, the degree of confirmation will be either unity or nil; intermediate values will indicate only the portion of the universe examined. Second, the procedure thus provides no differentiation between confirmation relative to a small as opposed to confirmation relative to a large restricted universe. Third, since diversity among the instances is irrelevant here, the second point means that this procedure provides no basis for preferring one evidence universe to another. In sum, metricisation here is mere window-dressing, and the confirmation procedure which results is mere toying. This plus the paradoxes is sufficient to disallow any attempt to base degree of confirmation of an hypothesis simply on the proportion of non-disconfirming instances to total instances in a restricted universe used as evidence.

It must be pointed out that the interpretation of the confirmation calculus I have used does not encounter such difficulties. It is true that application of 4 here also presupposes that no *c* instances are found, but this does not mean that the hypothesis has a probability of 1. Absence of *c* instances is no guarantee that the *a* or *d* instances found will be eliminative, and *a fortiori* no guarantee that all members of A' save G or A'' save F will be eliminated. It follows that none of the foregoing objections affect this approach.

Nonetheless, it may be replied, this kind of thing does indicate a failure to attain a uniform interpretation of the confirmation calculus that would permit its application regardless of whether the evidence is favourable or unfavourable. Furthermore, it is not clear that this interpretation is applicable to hypotheses any more complex than 1, nor is it clear that the metricisation is generally satisfactory. Finally, what of the views of Pears, H. G. Alexander, and J. L. Mackie? While all these follow Hosiasson-Lindenbaum's lead, they do not do so slavishly, and in particular do not make use of any formal confirmation calculus. Given the above criticism, this last point might be quite important.

The first step to be taken in response to this multipart objection is to ask what happens to the foregoing paradox solutions if we abandon metricisation. The answer, briefly, is nothing. The non-metricised parallel to the confirmation theory I have used to remove the paradoxes is, obviously, eliminative confirmation. Since it is the eliminative feature that serves to remove the paradoxes, the solution here proposed loses nothing by removal of any metricised elements. It would, of course, be a mistake to claim that this shows eliminative confirmation to be a satisfactory procedure; to do this here is impossible. Nonetheless, two observations are in order. First, if we abandon only the metricisation and not the view that the hypothesis is more or less confirmed relative to a given body of evidence, then no hypothesis can be said to be certain in the sense that it is confirmed beyond possibility of falsification. Second, it is not necessary to make any great sweeping assumptions, postulates, or what have you, to the effect that nature is uniform or that every event has a cause. All we need do is assume, for each hypothesis, that there is a correlation of the *sort* it specifies. In terms I have used before, for example, if we wish to eliminate members of the set A'' in an effort to confirm some member of that set as a sufficient condition of G, all we need assume is that there is one such property in the set. Then, the more members of A'' we eliminate, the higher the confirmation of the remaining ones as sufficient conditions; e.g. if F is not eliminated but other members of A'' are, 1 and 2 are better confirmed. Further, the set A'' can be expanded to include the various conjunctions that can be formed from its single members, thus making it possible to confirm that G has a sufficient, albeit complex, condition. Finally, it is worth noting that the assumption that there is a sufficient condition of G in A'' is not itself beyond falsification and elimination; it can be ruled out by eliminating all the members of A'', singly and conjointly, as sufficient conditions. If this happens, then we can obtain another set of initially possible conditions and go through the procedure again. Parallel conditions hold for the initial set of properties A' and the hypothesis that F has G as a necessary condition. All this can also be applied to the metricised confirmation procedure I used initially.

On the other hand, the non-metricised parallel to Hosiasson-Lindenbaum's approach is induction by simple enumeration. While abandonment of metricisation means that the built-in-equivalence criterion of the confirmation calculus goes, it does not mean that the equivalence criterion goes. As von Wright has observed, 1 and 2 are the same hypothesis [1]; since each entails the other, it is absurd to say that what confirms the one does not confirm the other. But this means that *a* instances confirm if and only if *d* ones do, and thus the previous criticism of Hosiasson-Lindenbaum's view on this score remains: we still have massive confirmation of the hypothesis in question by massive sets of *d* instances. If any non-disconfirming instance is thereby a confirming one, we also have confirmation by *b* instances. In short, neither the second nor the third paradox is removed. Again, the situation is not altered by appeal to some non-metricised formalisation of simple enumeration, e.g. Hempel's. Though his system represents an advance upon informal simple enumeration due to the attention to requirements of consistency and to relations among statements such that confirmation of one is confirmation of another, it remains the case that the underlying concept of the confirmation relation on which it is based is one which includes *b* and *d* instances as confirming without restriction.[2] Thus this is simply the formal door to the paradoxes. In short, the situation with regard to simple enumeration as a confirmation procedure comes down to this: if this is an admissible procedure, anyone can confirm practically anything by observing virtually anything. Nor is this significantly altered by requiring, e.g. that the evidence include one *a* and one *d* instance. Nor, again, is it significantly altered if we remove the second paradox by refusing to count as confirming instances those which falsify the converse of the hypothesis in question, i.e. refuse to count *b* instances as confirmations, since the *d* ones remain—massively. Consequently, simple enumeration must be abandoned, and this means, *inter alia*, that despite Nicod's claim [3] such enumeration can *not* be used as the basis for any other confirmation procedure, including elimination, whatever.

This last consequence of the confirmation paradoxes is no small matter, and thus it is of some importance to see whether or no the complete collapse of simple enumeration can, despite the previous paragraph's claims, be averted by appeal to some views which have been no more than mentioned so far in this paper. These are the developments, partially on the basis of Hosiasson-Lindenbaum's work, offered by Pears, Alexander, and Mackie. Again, however, the consequence can be put briefly: no such defence as is required can be found. This is so because, though none of these three makes use of anything like Hosiasson-Lindenbaum's formalisation, all use that element of her view which breaks down as a solution of the third paradox. All three argue that *d* instances are paradoxical confirmations because the conditions specified in formula 5 above (usually) hold, i.e. there are more $\sim G$'s than *F*'s. Thus, almost invariably, *d* instances are more probable than *a* ones, so the latter are better confirmations. Pears varies the argument somewhat by arguing that one runs more risk of falsification by looking for *a* instances than *d* ones, again because the latter are more likely, and that for this reason *a*'s are better confirmations.[1] So long as the instances are taken singly, this is all quite true. The difficulty is that this conclusion is inadequate, since no matter how slight may be the confirmatory value of any given *d* instance, such *do* confirm and enough confirm enough. The argument would remove the third paradox only if it established that *d* instances do not confirm at all, and this it does not do. Nor does appeal to Mackie's 'inverse principle' that an instance is a confirming one when addition of the hypothesis at issue to the evidence increases that instance's probability, change this; such addition raises the probability of *d*'s.[2] More, it may well be the case that it is more feasible, more economical, or more efficient to collect *d* instances in large numbers than to collect *a* instances at all, so that not even pragmatically is the third paradox removed. In short, these views give the appearance of escaping the third paradox only by committing a fallacy of composition. Pears commits the same mistake with regard to the second paradox. Once more we have confirmation of virtually anything by anybody almost anytime.

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[1] Von Wright, *Problem*, p. 127; cf. Hempel-1, p. 127, and -2, pp. 12-13

[2] Hempel-1, p. 130

[3] Nicod, op. cit., pp. 9-11, 16-19

Despite all this, Mackie may yet not be convinced, for he would reply that this massive confirmation by massive sets of *d* instances is an improper extension of instantial confirmation in bare contexts. It is, on his view, permissible to use *d* instances to confirm in bare contexts, i.e. where we have no background information such as that concerning the relative sizes of the class of *F*'s and that of $\sim G$'s. Most contexts, however, are not that bare; we do know the relative sizes, or at least know that there are far fewer *F*'s.[3] Unfortunately, this argument has the same conclusions as that resting on the 'inverse principle' and is similarly not strong enough. It would be a mistake to talk of massive confirmation by *d* instances only if the background knowledge made it impossible for these to confirm, but this it does not do. Once more, enough *d*'s confirm enough. Nor will it do to say that the appeal to relative sizes of classes here at least removes the second paradox, for it does not do this in some cases. It is true that, if we know how many $\sim FG$'s there are, how many things are *G*, and how many are *F*, we can decide 1 is false by computing that there are more *F*'s plus $\sim FG$'s than there are *G*'s. To do this, though, we need a finite universe, and one 'practically' so, i.e. where we can count up the various instances. Otherwise, the evidence is not sufficient, and the second paradox remains. The third paradox, of course, is not removed by such restriction to a finite domain.

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[1] Alexander-1, pp. 230-232; Mackie, op. cit., pp. 266-268

[2] Mackie, op. cit., p. 267; cf. Alexander-1, p. 232

[3] Mackie, op. cit., pp. 266-268; n.b. note 2. on p. 268

Finally, it is perhaps worth pointing out that, though Mackie, Pears, and Alexander also bring in the notion of attempted falsification, this will not save simple enumeration either. Alexander's point, redeveloped by Mackie, that *a* instances are better confirmations because a proportion of them markedly higher than the number we should expect if the hypothesis (1 or 2) were false tends to falsify the assumption that the hypothesis is false, is yet another case of the argument that does not prove enough. A large group of *d* instances could serve equally well, as Alexander admits.[1] Again, Mackie's point, that the assumption that we are looking for falsifying instances changes the likelihood of our finding *a*, *b*, or *d* ones, does not serve.[2] Even if we grant that such looking for *c*'s reduces the likelihood of finding any of the other three sorts, it remains the case that all three are so far still candidates as attempted unsuccessful falsifications. Worse, if their reduced likelihood makes them better confirmations, which is the relation essential to all the arguments of those following Hosiasson-Lindenbaum, then to adopt a policy of looking for falsifying instances is to compound the paradoxes by making *d* and *b* instances stronger confirmations than they otherwise are.

Before turning to some further relevant aspects of attempted unsuccessful falsification, it is well to summarise the rejection of simple enumeration here. This procedure turns on the rule that the more instances we have found which fit the hypothesis, the better confirmed that is. As elaborated by Mackie and Alexander, this is so qualified that it usually takes many *d* instances, or *b* ones, to achieve the confirmatory effect of a few, perhaps but one, *a* one. Nonetheless, such instances *do* confirm; they are not neutral ones. My dis-ease at finding that on the basis of simple enumeration each cord of wood the local woodsman cuts is evidence for 'all ravens are black' is not removed by my being told that he has to cut a lot of wood before we have any significant evidence. It is just nonsense to suppose this is evidence here at all, and simple enumeration is committed to this nonsense. It is because such confirmation is possible on the basis of simple enumeration that it permits the confirmation of practically anything by anyone at anytime; the elaborations suggested do not alter this. They simply require that we count higher. It is for this reason that I hold that simple enumeration must be rejected, no matter how this may affect eliminative confirmation. I think, though, that the latter can get on perfectly well without it, but this is another argument.

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[1] Alexander-1, pp. 230-232; Mackie, op. cit., p. 267

[2] Mackie, op. cit., pp. 274-275

What, then, of the further relevant aspects of attempted unsuccessful falsification? Strictly speaking, introduction of this as an element of a confirmation procedure is abandonment of confirmation by simple enumeration. More, it is worth repeating Alexander's point that such an element is not an automatic cure for the paradoxes since this procedure also must deal with the equivalence criterion. Other points of his which bear repetition are that what constitutes attempted unsuccessful falsification of a low-level hypothesis is not clear, and that this cannot be specified in terms of our expectations.[1] The lack of clarity is not removed by Mackie's policy of looking for *c* instances, for this does not tell us what counts as looking but not finding. Mackie fails to provide criteria for a procedure's being a genuine attempt to falsify an hypothesis. Again, neither such specification, nor the clarity sought, nor the removal of the paradoxes can be provided by appeal to the order of our knowledge. It cannot be held that only things known to be F or $\sim G$ before they are known to be G or $\sim F$ respectively are attempted unsuccessful falsifications. Von Wright's attempt to specify possible falsifying instances in this way and thus remove the paradoxes has been shown inadmissible, and the same arguments can be applied to the similar attempt by Watkins.[2] One by-product of this is that, since the whole appeal must be dropped, Mackie's interesting showing that the order of our knowledge does not affect the total confirmatory value of an *a*, *b*, or *d* instance though it does affect the value of the constituent parts of such instances is not requisite.[3] The net result of all this is that what is to count as an attempted unsuccessful falsification must be specified in terms of the relations among the instances and between them and the hypothesis. Moreover, use of such specifications to evade the paradoxes is abandonment of the argument from the relative probabilities of *a* and *d* instances, and may permit *d* instances as attempted unsuccessful falsifications.

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[1] Alexander-1, p. 229; -2, p. 234

[2] For Watkins's attempt, see Watkins-1, p. 319; for von Wright's, his *Problem*, pp. 122-127. For the criticism, see my 'Von Wright's Paradoxes', *Philosophy of Science*, 1963, 30, 165-172

[3] Mackie, op. cit., pp. 270-273

How does this relate to the paradox solution here proposed? What is about to be suggested will undoubtedly be received as a Trojan horse, if not worse, by the proponents of attempted unsuccessful falsification. The proposal is simply this: the conditions for attempted eliminative confirmation of various properties as sufficient or necessary conditions of another property are stated in terms of the properties of the instances, certain relations among the instances and between these and the hypothesis. If the same conditions can be used to specify what constitutes attempted unsuccessful falsification, and I think this can be done, since elimination is falsification and thus attempted unsuccessful elimination is attempted unsuccessful falsification, then the paradoxes will be removed by this procedure just as they are by eliminative confirmation. These few observations may at least partially dispel the odour of Greeks and sawdust: first, eliminative confirmation is here viewed as an inductive logic, not an heuristic procedure, not a recipe for baking scientific theories, not a description of the way scientists do in fact report their investigations (any more than deductive logic is descriptive), and not a method for finding true generalisations by sifting a mass of indiscriminately collected evidence. Second, as noted above, no claim to certainty in the inadmissible sense is possible; the most that can be said is that on the basis of the evidence presently in hand the hypothesis is maximally confirmed. Third, I cannot escape the impression that advocates of attempted unsuccessful falsification simply neglect some very large differences between enumerative and eliminative confirmation; it is not the case that all the difficulties facing the one face the other. However this proposed identification of eliminative confirmation and attempted unsuccessful falsification is greeted, the paradox solution based on the former is unaffected. But then, I do not wish to argue that it is the only possible solution; how, indeed, would one argue this at all?

There is one criticism of what has been done in this paper that must yet be considered; no definition of 'confirming instance' has been given, but according to von Wright this is an important part of any confirmation theory.[1] Though this discussion is not offered as a complete confirmation theory, I will not use that as a shield here. Instead,

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[1] Von Wright, *Problem*, p. 122

I wish to show that the demand for an explicit definition of 'confirming instance' is illegitimate since attempts to fulfil it have been a main source of the confirmation paradoxes. Why do we find paradoxical confirmations paradoxical? Some are so because they are instances which confirm but are not confirming instances; others are, for they are confirming instances which do not confirm. When I say here that an instance confirms, I mean that it does in fact add to the degree of confirmation of an hypothesis (whether or not this can be measured). When I say that an instance is a confirming instance, I mean that it qualifies as such according to one or another definition of that phrase. Thus, when some instance adds to the degree of confirmation of an hypothesis but is not a confirming instance, e.g. *d* instances for 1 for Nicod, or is a confirming instance but does not add to the degree of confirmation of an hypothesis, e.g. *b* instances for 1 or 2 for Hempel and von Wright, we have paradoxical confirmations. The equivalence criterion is certainly not to be broken or denied, but we could forget it and still envisage these kinds of paradoxical confirmations. The only way to prevent them is to ensure agreement between the definition of 'confirming instance' and the criteria for adding to the degree of confirmation of an hypothesis. But the only way to do this is to make these the same. In short, the way to define 'confirming instance' is not to attempt an explicit definition at all, but rather to say that this phrase is implicitly defined by the interpreted confirmation calculus or formulae or rules.

Two comments on this abandonment of any attempt to define 'confirming instance' are in order. First, it represents a notable advance over the von Wright-Hempel definition of a confirming instance as any non-disconfirming one. Such a definition not only leads directly to the second and third paradoxes; it also involves holding that there is no such thing as a neutral or irrelevant instance for any hypothesis. To try to remedy this by terming confirming instances that do confirm genuine confirming instances while terming those that do not confirm trivial or paradoxical ones is to compound oversight with confusion.[1] Second, it does not obviate the need for the sort of full-dress removal of the paradoxes presented in this paper, for the paradoxes are not entirely removed by this refusal to attempt definition. As is obvious from the examination of Hosiasson-Lindenbaum's view and Watkins's criticism of induction, that version of the third paradox which I have referred to as massive confirmation by massive sets of *d*

[1] Von Wright does this in his *Problem*, p. 127

instances can appear, as can the first and second paradoxes, and as can bogus confirmation by *a* instances, even when no attempt is made explicitly to define ' confirming instance '.

In sum, then, what has here been presented is a version of eliminative confirmation as an interpretation of von Wright's confirmation calculus, specifically his principal theorem of confirmation. On this basis the three paradoxes of confirmation have been shown either to disappear or to become innocuous. The approaches of Hosiasson-Lindenbaum, Alexander, Mackie, and Pears have been shown quite unsuccessful as attempts to remove the paradoxes. Further, I have argued that abandonment of metricisation makes no difference here; eliminative confirmation does, while simple enumeration does not, escape the paradoxes. This is taken as adequate grounds for rejection of simple enumeration as a confirmation procedure. While it is suggested that attempted unsuccessful falsification may escape the paradoxes by being at least partially identified with eliminative confirmation, this has not been elaborated. Finally, it has been pointed out that the demand for an explicit definition of ' confirming instance ' is mistaken. In closing, I wish to emphasise that I do not regard this as a full-scale defence or vindication of eliminative confirmation. While an occasional remark has been made in answer to one or another criticism of such confirmation, the central purpose here has been simply to show that one merit of it is that by use of it the confirmation paradoxes disappear.

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ON THE CONFIRMATION PARADOX*

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PROFESSOR MACKIE's discussion[1] of the confirmation paradox, or paradox of the ravens, attempts to relate the various types of solution which have been proposed so far. However I feel that an important type of solution has been overlooked by Mackie and other writers. Having observed the feathers flying in some of the wordy battles of the past I enter the field with some trepidation. However I feel that this new type of solution emphasises an important feature of scientific hypotheses which has not so far received sufficient attention. Mackie states that "no one will deny the equivalence of the two universal propositions' namely

'all ravens are black' (1)
and 'all non-black are non-ravens' (2)

but this is precisely what I propose to do.

Let me admit at once that if (1) is considered only as a proposition which is asserted to be true, then (2) follows by elementary logical rules and similarly (1) is entailed if (2) is asserted. Within the framework of deductive logic they are indeed equivalent, but no confirmation paradox can arise, since a statement which is asserted as *true* in deductive logic is not subject to confirmation.

However the whole question of the blackness of ravens has arisen not so much in relation to deductive logic or from an ornithological interest in the birds themselves but to illustrate the principles which underlie the confirmation of any low-level scientific hypothesis. It is therefore appropriate to ask whether (1) and (2) are equivalent as inductively derived scientific hypotheses, for if not, the paradox may be resolved without violating the equivalence condition or denying Mackie's propositions (1) and (2). Although the paradox arose in connection with Hempel's criteria of confirmation, the point I wish to make applies not within Hempel's rather austere context of no background knowledge but rather in the wider contexts discussed by Mackie and others. It will be convenient to use Jeffreys's probability notation

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* Received 3. ix. 63

[1] J. A. Mackie, this *Journal*, 1963, 13, 265-277

in which a logical statement which is asserted to be true is associated with probability number one, while the probability of a scientific hypothesis at any stage of its development must be less than one.

Any meaningful scientific statement or hypothesis must indicate clearly the *range* over which it is applicable. If we suggest that ' the period of a simple pendulum varies approximately as the square root of its length *l* ' the scientist makes the mental reservation that this is valid only if the amplitude is small and the length is small compared with the radius of the earth. He may quite properly attempt to extend the hypothesis beyond this range (e.g. for large amplitudes) but subsequent testing soon indicates the appropriate range of validity of his hypothesis and his statement becomes

> ' the period of a simple pendulum varies approximately as the square root of its length provided that the amplitude is small and that the length is small compared to the radius of the earth '. (3)

His statement tells us nothing about pendulums of greater length or wider amplitude or indeed about any other periodic motion such as orbiting satellites. Thus when he attempts to confirm (3) he concerns himself only with the appropriate simple pendulums. If we consider as an alternative hypothesis

> ' Anything not exhibiting a period proportional to the square root of its length is not a simple pendulum ', (4)

this statement bears the same logical relation to (3) as (2) does to (1). As an inductively derived scientific statement however (4) is not equivalent to (3). It is based apparently on a rather improbable set of observations of ' things not exhibiting a certain period ' and the problems of its confirmation will be very different to those of (3) because the range of validity to which these hypotheses apply is very different. It is true of course that we frequently test a higher level hypothesis by testing various lower level hypotheses which are deductively derived from it (e.g. the inverse square law of gravitation may be tested by observing periods of orbiting satellites or even of simple pendulums) but we do not claim that the hypotheses are equivalent.

Here then is the vital point. By their very nature, inductive statements are not strictly equivalent unless they refer to generalisations from equivalent sets of phenomena and are applicable over equivalent ranges. Further, when we consider the confirmation of (1) and its relation to the confirmation of (2), we may expect the same number of instances of confirmation of (1) and of (2) to carry the same weight only if (1) and (2) are inductively equivalent in the precise sense defined above.

The point is well made if we examine the differences between statements (1) and (2) more closely. Statement (1) can only arise from the study of *some ravens*. By inductive generalisation the range is extended to cover the class of '*all ravens*'. If we take a given point in time the number of elements in this class is bounded but, if we include all past and future ravens, the class is unbounded but is 'countable' or enumerable in the sense that an infinite series is enumerable ; i.e. its cardinal number is $\aleph_0$.

On the other hand the range of statement (2) is the class *all non-black* and as an inductive statement it must arise from observations of *some non-black*. The class of all non-black is certainly unbounded and, unless we make some further qualification such as all non-black *things* or all non-black *birds*, the cardinal number of the class is that of the continuum C where $C = \aleph_0^{\aleph_0}$ which is greater than $\aleph_0$. Thus (1) and (2) are inductive statements about classes with very different numerical properties and these properties have considerable bearing on their instantial confirmation. To confirm a statement about an enumerable class it might be reasonable to investigate the elements of the class in the order in which they are enumerated. The tests would never be completed but at any stage we would know what had been achieved and what to investigate next. On any theory it should be possible to estimate some 'degree of confirmation'. On the other hand there is, by definition, no order by which we may enumerate a class whose cardinal number is C. In fact since $C = \aleph_0^{\aleph_0}$ we would in principle require to test $\aleph_0$ elements of the second class before achieving the same degree of confirmation of (2) as obtained by one successful test of hypothesis (1). This is the root of the confirmation paradox.

In practice of course the question does not arise since the difficulty of the cardinal numbers may be removed by replacing (2) by a statement such as

all non-black *birds* are non-ravens. (2′)

Although the class of all non-black birds is finite or enumerable, (1) and (2′) are statements based on different observations and valid over different ranges. To confirm (1) we must observe ravens while to confirm (2′) we must observe non-black birds. Since at any given moment the range of 'all non-black birds' may be very different numerically to the range of 'all ravens', confirmation of an equal number of instances of (1) and (2′) should not be expected to yield the same 'degree of confirmation' of each. By this I do not imply that degrees of confirmation are in the inverse ratio of the numbers of articles in the classes observed. The whole problem of 'degree of confirmation' is much more complex and must depend to a considerable extent on the actual details of the particular hypothesis under consideration. It is sufficient to establish that different inductive hypotheses are not necessarily equally confirmed by the same number of instances of confirmation.

Of course the problem associated with the relative numbers of non-black things and of ravens has not escaped the notice of Mackie and others. They attempt to explain the paradox by arguing that 'the observation of non-black non-ravens is not nearly so good a confirmation of (1) as the observation of black ravens'. While there is a connection between this approach and my own, I suggest that the crux of the problem lies not in the 'relative strengths of different instantial confirmations', but rather in the essential differences between hypotheses arising from the different ranges of observations to which they apply.

Only if (1) is confirmed absolutely (i.e. its probability as a hypothesis is unity) will (2′) have the same degree of confirmation. This follows since a probability of one can only apply to a statement which is asserted as true and in this case (1) is equivalent to (2) and hence (2′) according to the rules of deductive logic. Thus we have turned full circle. The paradox is resolved since, if (1) and (2) or (2′) are logical statements, no question of inductive confirmation arises; if they are regarded as inductive hypotheses they are not equivalent in the precise inductive sense.

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CONFIRMATION BY OBSERVATION AND THE PARADOX OF THE RAVENS *

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THE present paper considers the possibility that the paradox of confirmation, or of the ravens, can be resolved on other grounds than those which, to my knowledge, have so far been discussed. The problem of the confirming power of enumerated observations as opposed to that of test cases is not entered into here. The view taken here is rather that confirmation by observation requires that a prior condition be fulfilled in order that observations may have any power at all to confirm an hypothesis. This condition, which is rarely made explicit, pertains to the relation between the observational data and the permissible inductive inferences from the data ; and its relevance to this paradox is that it is of such a nature that observations which may confirm one hypothesis will be entirely incapable of confirming certain logically equivalent, but nonetheless different hypotheses. If this view is correct, then the paradox can be resolved by rejecting the last of the three propositions which constitute it.

Restating the problem, the paradox is derived from three propositions given as follows : [1]

Prop. (1) Observations of black ravens confirm ' All ravens are black '.

Prop. (2) Observations of black shoes, white swans, etc., are neutral to (i.e. do not confirm) ' All ravens are black '.

Prop. (3) If observations confirm one formulation of an hypothesis, they confirm any logically equivalent formulation.

Given these three propositions, which all appear to be valid, the paradox is then established in the following manner : To begin with, ' All non-black are non-ravens ' is logically equivalent to ' All ravens are black '. Then, since an observation of a white swan is an observation of a non-black non-raven, observations of white swans confirm ' All non-black

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* Received 22. xi. 63

[1] I have followed the account of this paradox given by J. L. Mackie in his article ' The Paradox of Confirmation ', this *Journal*, 1963, 13, 265.

are non-ravens ' in the same way that observations of black ravens confirm ' All ravens are black '. By (3), if observations of white swans confirm one of these hypotheses, then they also confirm the other. But by (2), observations of white swans do not confirm ' All ravens are black '.

I

It is clear at the outset that an empirical criterion for the observational confirmation of hypotheses enters implicitly into the statement of the paradox. For we are told that an observation of a white swan is an observation of a non-black non-raven, and *therefore* observations of white swans are related to ' All non-black are non-ravens ' in the same way that observations of black ravens are related to ' All ravens are black '. This reasoning is presented as if the criterion that is implicit here were self-evident. At the same time, it is clear that this criterion is used to consider the relation between the observations and the hypothesis to which these are connected in each case, and that it is the ground of the claim that the relation is the same in the two cases. And since, by Prop. (1), the relation in question is that of confirming, it follows that the *same* relation of confirming is being claimed to hold in both cases.

Now surely, the very fact that we are here presented with selected pairs, each composed of an hypothesis and confirming observations, implies at least the possibility that, according to the criterion being used, the *same* relation of confirmation may perhaps not hold between observations of white swans and ' All ravens are black ', or between observations of black ravens and ' All non-black are non-ravens '. Bearing this in mind, if we then consider Prop. (3), we find that it claims that if observations bear this relation of confirming to one of the hypotheses, they necessarily bear this relation to the other also. Yet while this relation was originally considered on the ground of an empirical criterion, Prop. (3) simply ignores the matter of this criterion for the relation of confirming by observation.

The result is, then, that if we grant that there was an empirical criterion for considering earlier that certain observations confirmed certain hypotheses, it now becomes doubtful that the word ' confirm ' in the consequent of Prop. (3) means the same thing here that it meant earlier. That is, either we are being asked to accept the use of the word ' confirm ' in the consequent of Prop. (3) on the formal deductive ground of the logical equivalence of the propositions, in which case the meaning is not the same ; or, it is being assumed that the empirical criterion is automatically complied with. For unless this criterion is complied with, it is hard to see how the same relation of confirming that was considered earlier can be transitive with respect to all logically equivalent propositions. At the same time, it is hard to see how there can be any formal, logical necessity that this relation be transitive in this way, for the concept of it pertains to the methodology of empirical science, not to formal logic. And if there is any difference between logically equivalent propositions which may prevent an empirically defined relation of confirming from always being transitive with respect to them, then Prop. (3) is merely a different defining condition for confirmation, conceived from a formal point of view. That is, it can have no necessity in the sense that observational confirmation *must* be defined in such a way that it is transitive with respect to logically equivalent propositions. The problem is rather that of whether Prop. (3) is a defining condition for confirmation that is useful to empirical science.

The issue can be brought out by the following consideration : The scientist makes use of formal reasoning, within a theoretical context, to lead from an hypothesis that has been confirmed by one set of observations to an inference that is to be tested by different observations. If the inference is then confirmed, this supports the theoretical context. If it is not confirmed or is refuted, then the theoretical context enters into doubt. Now if Prop. (3) were valid, an immediate inference from a confirmed hypothesis to a logically equivalent one could never be useful in this way, because Prop. (3) has the consequence that it is impossible to differentiate between the observations which confirm two such hypotheses. Therefore, if it can be shown that the scientist uses an empirical criterion which clearly differentiates between such observations, and sometimes with extremely important results, then Prop. (3) is an unacceptable condition for confirmation.

2

If Prop. (3) were acceptable, this would mean that it is irrelevant which of a set of logically equivalent hypotheses a scientist sets out to verify by observation, because the same observations would confirm all of them. However, this is obviously not the case in the reality of scientific practice. The difficulty can be elicited from the problem of the ravens. We can see immediately that to confirm by observation that 'All ravens are black' gives rise to a different problem than that of confirming 'All non-black are non-ravens'. For in order to justify the induction of the first proposition from the data, enough ravens must be observed to support this generalisation about 'all ravens'. But to establish the second proposition by observation would require that enough non-blacks be observed to support this statement about 'all non-blacks'. In this case, the non-blacks that have to be observed may be limited to those that might be ravens, such as birds. But this does not affect the consideration that the terms of these propositions have reference to different things, and that they are thus different propositions, even though they are logically equivalent in the sense that they are reciprocally inferable from each other by the rules of immediate inference. Indeed, the very fact that each is the same accepted form of immediate inference from the other is sufficient to establish, on formal grounds, that these are different propositions. Because, if they were not in some way different from each other, it would not be possible to regard one as an inference from the other, since the mere restatement of the same proposition in different words is not an inference. And since these hypotheses differ in respect of the reference of their terms, it follows that if the reference of the terms is involved in the empirically defined relation of confirming, it cannot simply be assumed that this relation is transitive with respect to these hypotheses. It should also be made explicit, I think, that what is at issue here is not the reference of the propositions *as such*, but the reference of their terms to real things which are observable.

If it is conceded, then, that these two propositions stand to each other in a reciprocal relation of valid immediate inference, it must also be granted that the scientist who is setting out to confirm the black colour of ravens makes a real choice when he selects 'All ravens are black' as his observational hypothesis in preference to 'All non-black are non-ravens'. While, from his empirical point of view, the choice is by no means a matter of indifference, because the statement of an hypothesis that is to be confirmed by observation—and which is thus to be established inductively, from the data—is necessarily a statement about those things which have to be observed in order that the hypothesis should be confirmed.

This necessity, of course, has its only ground in scientific methodology. It simply expresses what an observational hypothesis is in empirical science, while the concept of confirmation asserts itself in the complementary aspects of the observer's two-fold search for the best hypothesis and the best confirming experience. It is this character of observational methodology which guarantees the precise specification of what is to be observed ; and, at the same time, it is a condition for the subsequent unambiguous statement of just what has been confirmed or unconfirmed by observation. That in practice there is always some observational imprecision or incompleteness does not affect the fundamental consideration that *the confirmation of an hypothesis by observation requires that the real things referred to in the hypothesis be actually observed.* Nor is this rule vitiated by the fact that it is often necessary for the observer to specify that his observations are of certain manifestations such as ' a flash on a screen ' rather than of ' an electron itself '. On the contrary, such cases serve to illustrate the need to distinguish between the precisely formulated observational hypothesis and some other hypothesis which will be regarded *as if* it were confirmed or unconfirmed by the outcome of the observation, on the grounds of a chain of reasoning connecting the two hypotheses. For it is just the preservation of this distinction between what is actually confirmed by observation and what is subsequently inferred that is the guarantee that traditional conceptual schemes, which are the ground of the subsequent inferences, will be subjected to critical analysis when they turn out to be inadequate to cope with the observational results. The assumption here, of course, is that observations do not contradict each other, but that, if the theoretical context is inadequate, inferences made within it from confirmed hypotheses can be refuted by observation. Moreover, this consideration is applicable not only to mediate but also to immediate inferences, because in practice immediate inferences are apt to be drawn in terms of a traditional conception of contrary properties, or sets of properties, that are viewed as being incapable of coexistence. Yet it is entirely possible that an immediate inference that is drawn in this way may fail at the hands of observational confirmation, or may even be clearly refuted. And if at the same time the original hypothesis continues to stand confirmed, then this places the conceptual context which is the ground of the inference in doubt. But evidently, this can happen only if the observations are different.

The example which comes immediately to mind is, of course, the case of the undulatory and corpuscular properties of the subatomic particles. If it is considered that the original hypothesis was ' All electrons manifest corpuscular properties ', then it could be inferred that, ' No objects not manifesting corpuscular properties are electrons '.

And since, by the classical concepts, objects which manifest certain undulatory properties are objects which do not manifest certain corpuscular properties just as surely as a white object is non-black, the inference was clearly refuted by the observation of the undulatory properties of electrons. At the same time, the original hypothesis is not considered to have been refuted, and this has placed the system of concepts, within which the inference was made, in doubt. For even those who claim to make it a matter of principle to retain these concepts have had to modify them in an important way by hedging them about with conditions with respect to their reference. Thus we have such various theories as that either the corpuscular properties, or the undulatory properties, or both kinds of properties are in some sense not the real properties of subatomic particles. And the result is that, in scientific statements about the processes on the quantum level, the terms which refer to these properties are no longer commonly understood to have an unambiguous reference to real things. In this case, then, the empirical criterion for confirmation, which determines what observations confirm or refute the respective hypotheses, has joined forces with immediate inference to reveal a real situation which our traditional concepts are inadequate to express.

3

I would like now to discuss an objection which has been made to my argument. It has been pointed out that in order to confirm ' All ravens are black ' and ' All non-black are non-ravens ', it is necessary in both cases to observe non-blacks that might be ravens, and the implication is that the observational problems are not clearly different. This is an important objection because if there were no criterion for establishing which observational *results* confirm which hypothesis, this would support Prop. (3). However, it seems to me that this objection rests just on assuming the absence of such a criterion. In addition, it has to be borne in mind that in order to reject Prop. (3), it is not necessary to prove that the same observational results can never verify the deduced truth values of related propositions. The problem is whether there is an independent empirical criterion for always being able to determine when the same observations do so and when they do not. I therefore wish to consider the objection from the point of view of whether the empirical criterion for confirmation can cope with it.

In the case of 'All ravens are black' it is clearly a part of the observational activity to look at non-black things that might be ravens, because if they turn out to be ravens, they will refute the hypothesis. Meanwhile, it has been granted that to confirm 'All non-black are non-ravens', the observations can be limited to non-blacks that might be ravens, in order to make sure that they are not ravens. But if they turn out to be ravens, then they will refute this hypothesis also, since they are non-blacks that are not non-ravens. There is an overlapping, then, of the observational problems. Yet in every case it is perfectly clear which results confirm or refute which of the hypotheses. Moreover, the overlapping does not concern securing the data that can *confirm* the respective hypotheses. For while the *same* observation of a non-black raven will refute both hypotheses, the *confirmation, as such*, of these hypotheses is achieved by *different* observations, i.e. non-black non-ravens and black ravens.

At first glance it might seem shocking that an observer is looking for black ravens to confirm 'All ravens are black'. It might be considered that he ought to keep an open mind and just look for ravens. But I think that this is an illusory sort of observational ethic that intrudes in this case only because it deals with a property like surface colour, which is easily observable whenever ravens are observable. However, if the observer were to advance the hypothesis, 'All birds are afraid of fire', it would clearly not be enough just to look for birds. He would have to look at birds in the presence of fire in order to be able to observe their fear, and he would have to arrange this. Furthermore, there are other kinds of properties than surface colour or fear of fire, instances of whose contradictories cannot be made to appear without varying the observational situation. To have an example of this we need look no further than the reactions of a given charged body in being attracted or repelled by other, different charged bodies. In the general case, if it is desired to confirm that an individual x has the property a, then x has to be observed under such conditions that, if it has the property a, it will manifest it under these conditions. In the case of the ravens, there are no special-conditions for their manifesting their blackness. Here we have a case of x's that manifest a property a whenever they are seen by the naked eye. That the observer is looking for their blackness to confirm his hypothesis is obscured by the fact that he does not have to make any special arrangements in order to observe it.

My point is that 'to confirm', as such, requires that specific confirming evidence be secured. And even in the simplest case the same observational situation is incapable of providing such confirming evidence of all logically equivalent hypotheses. For example, by bringing birds into the presence of fire, ' All birds are afraid of fire ' can be confirmed or refuted, and the same refuting observations would refute ' Beings not afraid of fire are non-birds '. But in order to confirm this latter hypothesis, non-birds have to be observed. On the other hand, to confirm ' All birds are afraid of fire ', the observer may look at some non-birds if he is doubtful about whether or not they are birds. But if he can he will first identify the birds, and he will bring only the birds into the presence of the fire, because these are the only observations that are relevant. In other words, a distinction has to be made between the observational activity and the relevant results, for not all the results are necessarily relevant. Thus whether the observer identifies the observed object before or after his experiment, only the observations of the things referred to in a given hypothesis can confirm that hypothesis. This is not to imply that the observer is not always on the alert for refuting evidence. But his long range objective is to confirm hypotheses in order to increase our knowledge. *Yet even though his hypothesis may be confirmable, and no refuting evidence appears, unless he secures the confirming evidence he will never establish it by observation.* Clearly, without the innumerable observations of black ravens, ' All ravens are black ' would not have been confirmed. And while the results which confirm ' All non-black are non-ravens ' may have been secured as a part of the activity which led to confirming ' All ravens are black ', they did not confirm this latter hypothesis but *only* failed to refute it. That is, they were not a test of it in the sense that ravens were observed and they did not turn out to be non-black.

In general, then, the empirical criterion is always adequate for determining which observational results confirm which logically equivalent hypotheses. It therefore seems possible to state that, given an hypothesis confirmed by observations and an immediate inference which contains the contradictories of the terms of the original hypothesis, the same observations which confirmed this latter do not confirm the inference. And since it is possible to define the relation of confirming in such a way that Prop. (3) is not valid, it seems clear that Prop. (3) has no force of formal necessity. Conceived from a formal point of view, Prop. (3) seems to me to be an unjustified extrapolation from the formal consideration of logically equivalent hypotheses to the empirical consideration of the confirming power of the same observations with respect to different hypotheses. This extrapolation might be said to confuse the deduced truth value of an immediate inference, which depends on the truth value of the original hypothesis, with the observational confirmation of the inference, which depends upon observing the real things referred to by the terms of the inference. Viewed as a defining condition for confirmation, Prop. (3) takes for granted the adequacy of the conceptual context within which immediate inferences are drawn. And surely this is to deny the function of observational confirmation to be a check on our conceptual systems.

4

The same argument which challenges the validity of Prop. (3) supports Prop. (2). Indeed, Prop. (2) is a statement of a consequence of the empirical criterion for confirmation. That is, observations of other things than ravens, whether or not they are black, do not confirm ' All ravens are black '.[1]

The problem of eliciting the empirical grounds for the validity of Prop. (2) as thus stated is, it seems to me, that of showing that any attempt to reject it involves the assumption of a distinction between observations which really confirm, so to speak, and observations that are merely compatible. In other words, the observational confirmation of ' All ravens are black ' cannot depend upon the data of observations of black shoes, white swans, etc. For without the observations of black ravens, there is no *evidence* of its truth. Thus to consider that white swans in any sense confirm, implies such a distinction. But if such a distinction were admitted, this would render ambiguous the concept of the relation of confirming which is defined unambiguously in Propositions (1) and (2). Meanwhile, it is implicit in Prop. (2) that something more than compatibility is required of observations that in any sense confirm an hypothesis. And this something more, it seems to me, is precisely that such observations must confirm in the sense that what is observed is an instance of what is stated by the hypothesis. The problem, then, would seem to be that if one accepts the empirical criterion for confirmation, then Prop. (2) follows. To reject Prop. (2) is to be talking about some other criterion for confirmation, which does not require that the empirical criterion always be fulfilled. For it cannot be claimed that white swans are evidence supporting ' All ravens are black ', in the sense of the empirical criterion which finds its expression in the concept of *direct observation*. However, to accept Prop. (2) is certainly not to imply that compatible observations play no part in scientific reasoning. For the observation of a white bird that turns out to be a swan permits the scientist to continue on the assumption that all ravens are black. But if there were a rumour that a non-black raven had been seen in a certain region, the only way to confirm ' All ravens are black ' would be to go to that region and observe all the ravens to be found there. What is at issue, it seems to me, is really the distinction between confirmation and compatibility. And that there is a special sense in which compatible observations are relevant to the problem of confirmation will be suggested in the next section.

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[1] It will be noted that the observations are not characterised in this formulation as being neutral. They that are not neutral is discussed in Section 5.

Before leaving the discussion of confirmation itself, I would like to consider one last point. The insistence on the empirical criterion should not, I think, be taken to imply that there is never any difference in the relations between hypotheses and confirming observations. For while the white swan satisfies the empirical criterion for confirming ' All non-black are non-ravens ', the data of observations of white swans do not give rise to the terms ' non-black ' and ' non-ravens ' as the data of observations of black ravens do give rise to the terms ' black ' and ' ravens '. This consideration seems to me to reveal a difference between these hypotheses in respect of their possible origins. ' All ravens are black ' is suggested by the observations themselves and is clearly an inductive generalisation. ' All non-black are non-ravens ' can then arise from it as a deductive inference and can be proposed for observational verification. So that we have an example of the power of immediate inference to result in observational hypotheses having negative terms. And whenever such an hypothesis is tested by observations of the things referred to by the negative terms, such observations are not only relevant to the problem of confirming the inference but can also give rise to some other hypothesis having positive terms. Thus while the observations which confirm ' All non-black are non-ravens ' do not confirm ' All ravens are black ', their confirming power is by no means limited to the first of these hypotheses.

This aspect of the problem seems to me to be overlooked by I. J. Good's notion of a statistician's stooge. His stoogian observer gives only ' yes ' and ' no ' answers to the statistician's questions as to whether or not the things observed are ravens and whether or not they are black. Thus when the stooge answers 'No,' to both questions, he is actually not providing the statistician with any confirming data of 'All non-black are non-ravens', but is merely stating that 'it is a case' of such confirmation. There would be no such entry in the statistician's non-black non-raven column as 'white swan', and there would be no way of attributing a reference to the marks in this column. And therefore—if I understand Good's suggestion correctly—there would be no data in this column, but only the number of observed facts that were not recorded. The facts themselves, with their power to confirm other hypotheses, would be lost. From the point of view in which the possible future usefulness of such facts is taken into account, this could surely not be considered an economical procedure. And in overlooking this aspect of the problem, the conception of the stoogian observations seems again to illustrate the neglect of the potentialities of immediate reference which is associated with accepting Prop. (3).

5

In conclusion, I would like to try to present an extremely interesting aspect of the problem of Prop. (2) which seems to me to open the whole matter of the empirical meaning of compatibility, in the context of the relations between observations and an hypothesis. Observations of white swans and black shoes have been regarded here as compatible, not neutral. For to consider that such observations do not confirm 'All ravens are black' is not to assert that they are entirely neutral, or irrelevant, to this hypothesis. On the contrary, from the empirical point of view, these compatible observations establish the prior conditions for proposing this observational hypothesis.[1] That is, the observer not only assumes that ravens are observable objects and that black is an observable property, but he also takes as verified two other conditions: first, that there are other black objects than ravens, '. . . so that you cannot identify a raven merely by seeing something black; and, on the other hand, that blackness is not a general property of things, so that the observation of blackness is relevant,' i.e., relevant to specifying the properties of ravens as distinct from some other things with which they might be confused.

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[1] I owe this illuminating suggestion to Mr N.W. Pirie, whom I quote in what follows. Mr Pirie very generously gave his time to read an earlier version of this paper, and to point out to me just why the neutrality of observations of white swans and black shoes should be rejected. This is not to imply, of course, that Mr Pirie's view of the paradox of confirmation is in all other respects the same as mine.

The making explicit of these conditions for asserting the observational hypothesis, ' All ravens are black ', puts the whole matter of Prop. (2) in a new light. For what becomes very clear is that observations of such things as black shoes and white swans are implicit in the very formulation of this hypothesis. To be convinced of this, one has only to consider what ' All ravens are black ' could mean if the verification of one or the other of such kinds of observations were not implicit. If no such things as black shoes had ever been observed, the hypothesis could mean that all ravens are all black things. In other words, it would be just as reasonable to assume the distributed predicate as it is reasonable in the world as we know it to assume the undistributed predicate. And the result would be that observations of black shoes would refute the hypothesis. In this case, then, we would be dealing with an Aristotelian ' property ' pertaining to all ravens and to nothing else. On the other hand, if no such thing as a white swan had ever been observed, then the hypothesis could mean that all ravens belong to the class of things possessing the one known colour. In such a world, it might be assumed that to refute the hypothesis it would be necessary to observe a raven not possessing colour. But in such a world it might be more reasonable to interpret ' All ravens are black ' as a mere assertion of material existence.

These considerations are offered here in a very tentative spirit. The problem involved seems to be the large one of analysing what kinds of compatible observations are implicit in the assertion of any specified observational hypothesis. At the same time, it does not seem to me that this rejection of the neutrality of the white swans and black shoes implies that they confirm ' All ravens are black ', as claimed by Prop. (3), but rather that their non-neutrality, so to speak, is the content of their compatibility. For to say that to assert a given observational hypothesis implies that certain classes of objects other than those referred to in the hypothesis are not empty, is not to say that observations of those other objects confirm the hypothesis. Such observations merely establish that in this kind of a world, the problem of confirming this hypothesis is unambiguously understood. In this case, for example, the quantity of such observations is irrelevant. To propose ' All ravens are black ', all we have to know is that there are *some* non-black non-ravens and *some* black non-ravens. That ' All non-black are non-ravens ' is not at issue, while ' Some blacks are non-ravens ' does have to be assumed. Therefore these conditions seem to me neither to imply Prop. (3) nor to be explicable by means of Prop. (3), but rather to give rise to a new aspect of the problem of the concept of observational hypotheses.

One might say, perhaps, in a very general and preliminary way, that the formulating of an observational hypothesis has two aspects. On the one hand, it is formulated so as to be confirmable by observations of the real things referred to in it, in the sense described in the earlier part of this paper. On the other hand, it is formulated so as to be compatible with the already verified observations concerning the universe of discourse implied by the hypothesis. When put in this way, there is of course nothing new in this last statement. But in the context of the problem of confirmation, such compatible observations are what make possible the unambiguous interpretation of the hypothesis. And by making this explicit it becomes possible to reconcile the strict empirical concept of confirmation by direct observation with the undeniable relevance of the larger context to any hypothesis proposed within it.

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A NEW APPROACH TO THE THEORY OF FUNDAMENTAL PROCESSES*†

MENDEL SACHS

1 *Introduction*

IN his fundamental treatise, *The Science of Mechanics*, Mach [1] stated:

> We fill out the gaps of experience by the ideas that experiment suggests. Yet, not all the prevalent scientific theories originated so naturally and artlessly. Thus, chemical, electrical, and optical phenomena are explained by atoms. But the mental artifice *atom* was not formed by the principle of continuity; on the contrary, it is a product especially devised for the purpose in view. Atoms cannot be perceived by the senses; like all substances, they are things of thought. Furthermore, the atoms are invested with properties that absolutely contradict the attributes hitherto observed in bodies. However well fitted atomic theories may be to reproduce certain groups of facts, the physical inquirer who has laid to heart Newton's rules will only admit those theories as *provisional* helps, and will strive to attain in a more natural way, a satisfactory substitute. The atomic theory plays a part in physics similar to certain auxiliary concepts in mathematics; it is a mathematical model for facilitating a mental reproduction of facts.

The contemporary approach to the description of elementary particle systems has not yet unified the underlying notions of Einstein's theory of relativity with those of quantum mechanics. In searching for a path that may lead toward such a unification, a new approach to the theory of fundamental processes has been developed. The *philosophical aspects* of this approach are in accord with the view of the atomic theory

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discussed above by Mach.[1] It is the purpose of this paper to discuss the underlying notions of this new approach along with its points of departure from the present-day theories.

Around the beginning of this century, two sets of experiments revealed contradictions with the existing physical theories. One of these was the observed spectrum of black body radiation—the radiation emitted by a hot gas that is in thermodynamic equilibrium with its own radiation field. The other experiment was the Michelson-Morley investigation of the velocity of light through the ether, as a function of its direction relative to the direction of rotation of the earth. To explain the results of these experiments two revolutions in physics and in the philosophy of science were born. The former experimental observation led Planck to the concept of *discreteness* of physical observables. The logical course of the ensuing theory then led to the nondeterministic description of measurement that is embodied in present-day quantum mechanics. The latter experiment led Einstein to re-examine the foundations of Newtonian mechanics, thus leading to relativistic mechanics—a theory that is based on the postulate that the laws of Nature should remain invariant under *continuous* transformations from the space-time frame of reference of one observer to that of another.[2]

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[1] It should be emphasised that the agreement which is referred to is with Mach's adherence to an underlying *continuity principle* and with his acceptance of the ' observed ' discreteness in microscopic systems only as a temporary auxiliary concept in the mathematical description of a restricted group of facts. The theory discussed in this paper is not necessarily compatible with all of the aspects of Mach's more general views of epistemology.

A. Landé (in *From Dualism to Unity in Quantum Physics*, Cambridge University Press, New York, 1960) also evokes a *continuity principle* in the description of microscopic phenomena. His concept of continuity, however, is in the sense of Leibniz's *cause-effect continuity*. With this as an underlying concept, Landé maintains the intrinsic probabilistic features of the quantum theory, including, for example, the necessity for the ' quantum jump '. In contrast, the approach of this author refers to the *principle of continuity* in the sense of a mathematically continuous description of Nature, along with the prediction of all observables in terms of a continuous distribution of values. In the language of Mathematics, the approach discussed in this paper requires that the set of observables that describe Nature, belong to a *compact function space*.

Others who have adhered to the *principle of continuity*, in the sense that is discussed by this author, were Einstein and Schrödinger (see, for example, E. Schrödinger, this *Journal*, 1952, 3, 109, 233).

[2] The technical term for this imposition on physical theories is *covariance*. In the special case when the space-time reference frames of two observers are relatively at rest or in uniform motion, the formalism is reduced to the *special theory of relativity*.

Along with their compatibility with the predictions of the classical theories (in the respective limits of microscopic dimensions and small velocities) the quantum theory predicted, quite successfully, the non-relativistic properties of atomic systems while relativistic mechanics accurately predicted the equations of motion of 'classical' systems in which the relative velocities are close to the velocity of light in a vacuum (i.e. the maximum velocity of propagation of information between two interacting objects). However, the underlying notions of quantum mechanics and of the theory of relativity remain incompatible.

The approach that is taken by Einstein is that the laws of Nature must be described in terms of a deterministic and covariant formalism that is based entirely on the *field* concept. On the other hand, quantum mechanics associates a field variable, $\psi(x_j)$, with a probability amplitude that accompanies the *trajectory* of each of the elementary particles (j) of the system that is described.[1] The probabilistic feature of the quantum theory is, according to Bohr's principle of complementarity, inherent in the measuring process as an objective entity. Quantum mechanics considers a measurement in terms of a state of the measured system that involves a distribution of elementary particle trajectories. According to the Heisenberg uncertainty principle, each of these trajectories do not, by themselves, have a complete dynamical description. This is because of the simultaneous nondeterminability of all of

While the discussion that is given in this paper is not restricted to this special case, it is the only case that has been investigated within the framework of this theory, thus far. Investigations of extensions to general relativistic covariance are currently in progress.

[1] The present-day approach to quantum mechanics has a few *equivalent* interpretations. The one that is mentioned here is similar to the approach that was taken by R. P. Feynman, *Rev. Mod. Phys.*, 1948, **20**, 367. One other interesting interpretation of the conventional formalism, that the reader may wish to compare with the theory that is proposed in this paper, is due to H. Everett, III, *Rev. Mod. Phys.*, 1957, **29**, 454. Everett attempts to arrive at the inherent probabilistic features of quantum mechanics through an interpretation which requires that the states associated with observables can only be considered as *relative* to the remaining states of a combined system, that includes the measuring apparatus. His approach continues to maintain the usual linear formalism of quantum mechanics and proceeds from its underlying principle of linear superposition. In contrast, the theory to be discussed in this paper, while incorporating the measuring apparatus, does so in the sense of a closed description in which the solutions of coupled field equations provide a complete (non-probabilistic) description of the combined system. Linear superposition may not be used here as an *underlying principle* because of the essential feature of *nonlinearity* in the field equations (although it may be utilised in special limits when the field equations *approach* a linearised form).

the dynamical variables which are involved in the individual particle description. According to the principle of complementarity, however, the lack of predictability of a dynamical variable (to some specified degree of precision) in one constituent element of the measurement is complemented by an increased precision in that variable in another element of the same measurement. Thus, the average result for the entire (macroscopic) measurement will always be the same and it therefore obeys the fundamental requirement of scientific reproducibility. Nevertheless, the incomplete description of the basic elements that make up the measurement yield a nondeterministic theory.

The logical consistency of this approach to the theory of measurement (which was first given a rigorous mathematical formulation by von Neumann [2]) has been debated for many years. [3, 4] These discussions, however, have usually been confined to the description of nonrelativistic phenomena, which have already been predicted, quite accurately, by the *formalism* of nonrelativistic quantum mechanics. It is not the purpose of this paper to analyse the pros and cons of these discussions. Rather, it is the purpose here to discuss a theory which grew out of the thesis that if the principle of relativity is accepted as an axiom that underlies all physical phenomena, then the quantum theory cannot yet claim to be established as a general theory, even though its formalism does make accurate predictions regarding a large group of facts. The reason for this contention is that a consistent relativistic quantum field theory has not yet been developed for the description of any phase of Nature.

The approach which is investigated here requires that the formalism and predictions of nonrelativistic quantum mechanics should follow, only as a limiting feature of a self-consistent and relativistically covariant field theory. One important point of departure of the proposed theory from the conventional approach lies in the acceptance of an underlying continuity principle. This is in contrast to the inherent discreteness of physical phenomena that is *postulated* by the quantum theory. Certainly, discreteness in physical observables has never been measured. All observations have involved a continuous distribution of values. In view of the ' peaked ' nature of the measured properties of microscopic systems, it is quite natural to extrapolate from the data to the so-called ' ideal case ' of discreteness. However, such an approach can be acceptable, within the framework of a logical development, only so long as the ensuing theory remains consistent while encompassing all observable phenomena and under all conditions. This requirement has not been fulfilled by the present-day quantum theory in its extension to encompass high energy phenomena.

The approach discussed here investigates the possibility of an underlying continuity principle, thereby assuming that the 'ideal' limit of discreteness is never in fact realised in Nature. The reason, to be discussed below, has to do with the requirement of a *closed* theory and the nonlinear aspects that emerge from a formalism which follows from a full exploitation of the principle of covariance.

One would expect that differences between the quantum theory and a deterministic and covariant description of elementary particles, which is based entirely on the field concept, should indeed be unimportant as far as the mathematical description of nonrelativistic phenomena is concerned. They should, however, be all-important in the formulation of the exact (relativistic) theory. The development of techniques for measuring high energy properties of microscopic systems necessitates the fusion of the results of quantum mechanics with the theory of relativity. Present-day quantum field theory attempts to do this, however still maintaining the underlying nondeterministic approach of the quantum theory. It has been shown by Bohr and Rosenfeld [5] to follow from the uncertainty principle that just as the dynamical variables (position and momentum) of a particle description are not simultaneously measurable quantities, so not all of the field variables to be used in the field description can be associated with simultaneously measurable quantities, within a sufficiently small region of space. Thus, for example, the quantum theory would predict that one cannot speak of the simultaneous measurement, to arbitrary accuracy, of all of the components of the electric and magnetic field variables which are associated with a moving charged particle. This is expressed in the mathematical formalism by representing the complete set of dynamical field variables by a nonabelian set of *field operators* (i.e. by imposing an algebraic structure in which not all of the field operators commute with each other). In this way the field theory becomes quantised. This mathematical procedure (called *second quantisation*) is then necessitated by the assumption that the Heisenberg uncertainty principle is basic to all physical phenomena. Thus, even in the case of a covariant description, the quantum mechanical approach requires a non-deterministic description of the measuring process. It is this description that forms the basis of the epistemological approach of the Copenhagen School.

Einstein [6] adopted the view that those fundamental elements which make up the whole observation and provide us with a knowledge of our environment, must necessarily have a complete description. From the standpoint of physics, both of these epistemological approaches would be equally acceptable (*a*) so long as they are each developed in a logically consistent way, and (*b*) if they provide equally satisfactory predictions to agree with experience. If both approaches are equally satisfactory in this way, then a choice could be made only in terms of intuition. It would be hoped that future experimentation and theoretical analysis would establish the initial choice because of agreement between the data and possible differences that may occur in the predictions of the different theories.

Let us now consider the present state of acceptability of the quantum theory of fields. The present-day approach to quantum field theory has had two remarkable quantitative successes. These deal with one of the known types of interaction—the electromagnetic interaction. This theory (quantum electrodynamics) has predicted quite accurately two effects that are not predicted at all by the ' first quantised ' theory. These are the Lamb shift (a property of the energy level structure of hydrogenic atoms) and the magnetic moment of the electron.

On the other hand, present-day field theory suffers from some inherently undesirable features. First, there are infinite quantities that appear in the formalism and must be removed by making additional physical assumptions and introducing additional mathematical techniques, before a comparison can be made with the *finite* observations. This is the so-called renormalisation procedure. Several authors have already demonstrated a lack of consistency in this scheme of prediction. [7][1]

Second, the theory does not predict any of the measured properties of the system in closed form. All of its predictions are inherently contained in perturbation expansions which express the effect of the *perturbing interactions* on *free particle fields*. From the physical point of view, this may seem a reasonable procedure when the experiment that one wishes to consider involves the scattering effect of one particle on another and when one observes the particles at times long before and long after the scattering event has taken place (especially when the scattering force has a very short range—such as nuclear forces). However, one may question the validity of such an approach to determine the properties of a bound system of particles (e.g. an atom or a nucleus). Further, the convergence of the actual expansions that are used to calculate physical observables are in question [8].

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[1] *Note added in proof.* Kallen's proof of inconsistency in the renormalisation scheme of quantum field theory has been partially invalidated in later work by S. G. Gasiorowicz, D. R. Yennie, and H. Surra (*Phys. Rev. Lett.*, 1959, **2**, 513). The investigation of the latter authors led to the conclusion that quantum field theory is not necessarily inconsistent—even though it has not been shown to be a mathematically consistent scheme. More recent investigations by J. G. Taylor (*Nuovo Cimento Suppl.*, in press) of reformulations of quantum field theory in which the infinities can be removed in a mathematically consistent way, as well as going beyond the standard perturbation formalisation, have indicated a possible improvement.

The formal difficulties of quantum field theory have been well known for many years. They have their roots either in the mathematical formalism or in the conceptual structure of the theory. Thus far, mathematical investigations have not satisfactorily resolved these difficulties. In the following sections of this paper, the essential ideas that underlie a new approach to the theory of elementary particles are discussed, taking as a starting point a reinvestigation of the conceptual structure of fundamental processes. The mathematical details, along with further discussion are contained in other publications. [9–12, 29, 33, 34.]

2 *Theoretical Approach*

The approach that is taken, similar to that of Einstein, assumes that the laws of Nature are described in terms of a deterministic, covariant and completely field theoretical formalism. In contrast to the present-day ' second quantised ' theory, the theory discussed here is a *c*-number field theory in the sense of the Einstein and Maxwell field theories. That is, for a system containing a finite number of particles, there are a finite number of (continuous and continuously differentiable) field variables, forming an *abelian* set

$$\{A^{(1)}(\mathbf{x}), A^{(2)}(\mathbf{x}), \ldots, A^{(n)}(\mathbf{x})\}$$

The independent variables $\mathbf{x} = (x_0, x_1, x_2, x_3)$ represent nothing more than a set of four parameters that vary continuously over a grid that is constructed to map the field variables of the theory. Thus, in accord with the underlying notion of the principle of relativity, the co-ordinate space is chosen strictly to facilitate the mathematical construction of the theory and not for the (subjective) purposes of ' pinning down ' the trajectory of the particle (or particles of a system) that we envisage in our mind's eye. Thus, all of the field variables of the theory are mapped onto the same four-dimensional co-ordinate system. A knowledge of such a mapping provides a *complete* description of the system. These functions are solutions of differential equations—the field equations—that we associate with the physical laws.

It follows from the invariance properties of the physical laws under transformations between the co-ordinate systems of different observers that certain quantities are conserved in time. These are the *constants of the motion.* In accordance with the principle of relativity, and in the special case when the relative space-time co-ordinate systems are inertial (i.e. when the two observers are at rest or in relative uniform motion) the invariance properties are prescribed by the *proper inhomogeneous Lorentz group*—a restricted group of *continuous* transformations in space-time.[1] Thus, it follows from the invariance of the physical laws under arbitrarily small translations in space-time that the components of the energy-momentum four-vector are constants of the motion. Similarly, invariance under arbitrarily small rotations in space-time imply that the components of the angular-momentum tensor are constants of the motion. These features of the ' classical ' field theory are emphasised here in order to point out how such observed properties of an elementary particle system will follow from a formalism that is in strict accord with the *principle of continuity.* This is in contrast to the present-day quantum field theory in which the discrete character of the physical observables is built into the formalism by requiring that not all of the field variables may commute with each other.

It is, of course, incumbent upon any fundamental theory to reproduce the experimentally observed features of atomic systems that are also contained in the predictions of nonrelativistic quantum mechanics. Since the latter predicts discrete energy levels and angular momenta as a consequence of linear eigenvalue equations and the interpretation of their solutions, it follows that if the *nonrelativistic limit* of a different theory were to have the same form in terms of linear eigenvalue equations and boundary conditions on their solutions, the proposed theory should not differ in its predictions about nonrelativistic systems with the conventional Schrödinger theory. The same argument holds for the relativistic problem in which one takes the limit where a part of the system becomes infinitely massive in comparison with the rest of the system. Such a case is that of the electron-proton system in which the field equations of the theory should reduce to a form that contains the standard Dirac equation for hydrogen, when the proton mass can be considered to be infinite, compared with the electron mass. Thus, it is the view of the new approach that is discussed in this paper that the apparent discreteness of atomic energy levels is not a fundamentally inherent property, it is rather a limiting form of a continuous entity.

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[1] Since the only types of transformations required by the principle of covariance to maintain the form of the field equations are *continuous* transformations, there is no special reason why the laws of Nature should be invariant under the *discontinuous* spatial and temporal reflections. The former invariance (parity conservation) has been shown to break down in weak interactions. [13-16] Implications of a breakdown of both of these symmetries in electromagnetic interactions have also been investigated. [17, 18] In all generality, then, the field theory that is discussed in this paper does include a lack of invariance under both spatial and temporal reflections.

So far, the discussion has been designed to point out differences between some of the essential features of the present-day quantum field theory and features of the so-called 'classical' field theory. The approach discussed in this paper is in accordance with the latter. Where it differs, however, both from the present-day theory and from Einstein's view [19] is in the role that is played by the elementary particle. The essential point of departure of this new approach is in the introduction of the following fundamental postulate:

> *The laws of Nature must be described in terms of field variables that may be associated only with elementary interactions.*

Thus, it is proposed that the *absolute* concept of the particle be replaced with the *operational* concept of the interaction as an elementary concept. The theory accepts the statement about the existence of a particle, only in so far as it can influence another particle in an observable way.[1] The field equations then relate to the manifestations of these particles in each other's presence and not to their intrinsic properties when they are isolated and free. For if there were only one particle in the universe, then nothing more could be said about it since there are no observables involved in the description. Thus, a statement about the existence of a 'thing', as a free particle, is a tautological statement. It requires at least two particles to make any statement about physical observables. It follows, then, that the most elementary system is a two-particle system. In this system, one of the particles may be called '*observer*' and the other, '*observed*'.

It is interesting to quote, at this point, from the writings of Berkeley. [20]

> It does not appear to me that there can be any motion other than *relative*; so that to conceive motion there must be at least conceived two bodies, whereof the distance or position in regard to each other is varied.

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[1] To clarify this point further, one may consider that the *passive* role played here, by the *particle field*, in relation to the *interaction field*, is analogous in mathematical analysis to the role that is played by the *point* in relation to the *line*.

> Hence, if there was only one body in being it could not possibly be moved. This seems evident, in that the idea I have of motion does necessarily include relation.

To exemplify this notion further, consider the following simple experiment in elementary electrostatics. Suppose that we are given two identical pith balls, i.e. these are dielectric spheres of equal size, mass and chemical constituency. Let the pith balls suspend by conducting wires from a common point. A contact that is made between this point and a charged rod will induce a repulsive interaction between the pith balls and we would thereby observe them to separate symmetrically from the vertical to a separation d. From the laws of statics and our knowledge that the pith balls each weigh the same, we can conclude from a series of such experiments, each time changing the weight of the pair of pith balls, that the force of repulsion which separates them is inversely proportional to d^2, i.e. $F = k/d^2$, where k is a proportionality constant, independent of d.

In analysing the results of this experiment (on Coulomb's law) the tacit assumption is usually made that there is, associated with each of the pith balls, an inherent charge q_i (that was imparted to it by the charged rod). From the symmetry of the observation it is then concluded that $q_1 = q_2$. Finally, it is concluded that the *observed* constant k is the product $q_1 q_2 = q^2$. Thus, from the measurement of the quantity q^2, one extrapolates to the property q_i of the i^{th} isolated, noninteracting pith ball.

It is clear that this extrapolation is not a unique one. Thus, an alternate conclusion that $q_1 = nq$ and $q_2 = q/n$ (where n is any real number) would lead to the same observed quantity. It is the contention of our approach that since the quantity q^2 (and not q_1 or q_2, separately) is related to the physical observable, it is this quantity that must be treated as the fundamental constant of the measurement. Thus, for example, an experiment that is carried out by increasing the number of charged pith balls on one side of the vertical, would lead to an observed change in the separation of the pith balls—an effect that is related only to a change in the interaction constant q^2, and not necessarily to a change in the intrinsic properties on the interacting bodies.[1]

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[1] *Note added in proof.* In the case of more than two interacting pith balls, it becomes necessary (in the usual description) to consider the magnitudes of the component vectors F_{ij} of the 'polygon of forces', with $F_{ij} \propto q_i q_j$. The lack of uniqueness appears here in the individual assignments of q_i and q_j within any given F_{ij}. Thus, it is possible that in F_{ij}, q_j can have one value and in F_{jk} (for all interacting pith

While the orthodox approach, which assigns inherent properties, such as the charge q_i, to the individual bodies that participate in the interaction may be logically consistent, to begin with, the consistency of this approach does indeed break down if we allow the macroscopic body to shrink down to the charged elementary point particle. Such a procedure, which is necessitated by the concept of a finite signal velocity of relativity theory [21], leads to the well-known infinite self-energy of the charged elementary particle. On the other hand, this difficulty does not appear in the proposed theory since the concept of self-force is inconsistent with the basic premise that takes the *interaction between particles* rather than the *free particle* as the fundamental building block of Nature. The self-energies of the conventional quantum field theories never appear in this theory.

In connection with the example discussed above, it should be re-emphasised that it is not the electronic charge e which is to be considered as a fundamental constant of Nature; it is rather the constant e^2, representing the strength of an elementary (electromagnetic) interaction. From experience, it is recognised that the elementary interaction is either attractive (characterised by $-e^2$) or it is repulsive (characterised by $+e^2$). The conventional extrapolation leads to the statement that elementary particles have either a positive or a negative charge (or that they are uncharged). The approach of this theory ends with the statement that the fundamental electromagnetic interaction is either attractive or it is repulsive, depending on the nature of the entire system of particles.

Let us now consider the development of the formal structure of the theory as a consequence of its axioms. According to the underlying premise of the fundamental nature of the interaction, the field variables that are determined from the basic equations of the theory do not relate to the intrinsic properties of the constituent particles of the system; they are rather related to the mutual influence that particles exert on each other. One then relates such a *mutual influence* to a *measurement*, associating some of the field variables ($\Lambda^{(1)}(\mathbf{x}), \ldots, \Lambda^{(m)}(\mathbf{x})$) with the *observer* and the remaining coupled field variables ($\Lambda^{(m+1)}(\mathbf{x}), \ldots \Lambda^{(n)}(\mathbf{x})$) with the *observed*, according to the nature of the problem under study.

In the two-particle system, one associates one of the field variables $\Lambda^{(1)}(\mathbf{x})$ with the *observer* and the other (coupled) field variable $\Lambda^{(2)}(\mathbf{x})$

balls $j \neq i,k$) q_j can have other values–while still maintaining the *observed* (and unique) magnitudes of each of the components F_{ij} of the polygon of forces to be proportional to $q_i q_j$.

with the *observed.* According to the principle of covariance, however, it should make no difference as to which particle is called *observer* and which is called *observed.* Thus, in strict accordance with this underlying principle, and the relationship of the field variables to *one interaction,* it is required that the mathematical structure of the physical laws must necessarily be symmetric with respect to an interchange of the field variables associated with the *observer* and the field variables of the *observed.*[1] Consequently, it is concluded that the description of the system must be *closed,* i.e. the measuring apparatus as well as the measured system are included in the description. This imposed property of the physical laws, called *self-consistency,* is a feature of this theory that represents an important point of departure from the epistemological approach as well as the formalism of the present-day theory of elementary processes.

With this description, the idea of ' collapsing of the wave packet ' in a measurement process, is not a meaningful concept. This is because the field variables of the formalism provide a complete and dynamical description of the entire *closed* system, thereby eliminating the need for ' quantum jumps ', at any stage of the measuring process.

The arbitrariness of the names ' *observer* ' and ' *observed* ' follows from the basic notion of the theory of relativity which takes the *observer-signal-observed* as the fundamental unit. Thus, the laws of Nature that are investigated by the *observer* are precisely the same as the laws of Nature that are investigated by the *observed* (who calls himself the *observer*) when their measurements are interpreted in terms of their mutual influence and nothing more. In this way, the principle of relativity leads to an unfolding of a purely objective description of Nature.

An important consequence of the imposed symmetry of a closed system is a set of corresponding field equations that are nonlinear.[2]

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[1] This permutation symmetry is one of the additional two groups of symmetry elements that must, in accordance with this theory, be adjoined to the proper inhomogeneous Lorentz group. The second one follows from the requirement of conservation of the sources of the interaction field. This conservation law follows from the imposition of invariance under the elements of the gauge group (i.e. invariance under gauge transformations [of the first kind] $\psi \rightarrow \psi e^{i\alpha}$).

[2] With interactions ' on ', the equations of the ordinary quantum field theory are also nonlinear in their initial construction. However, they are linearised by relating the solutions to a set of *nonabelian operators* and allowing them to operate on the imposed stationary state function for the whole system. This procedure (second quantisation) is not followed in the formalism of the theory discussed in this paper because of the entirely different interpretation of the field solutions.

The maintenance of this nonlinearity as an essential feature of the theory then forbids a description of Nature in terms of a *linear* superposition of states. Thus, the Hilbert space formalism of von Neumann's development of the quantum theory of measurement cannot be made compatible with the underlying notions of the approach discussed here.

It is clear that in the limiting case when the *observer* is macroscopic and the *observed* is microscopic, one can, to a high degree of precision, neglect the effect on the microscopic system that is due to its reaction to the presence in the universe of the macroscopic system. To exemplify this point, consider the measurement in which the macroscopic human brain, with its eye mechanism, is coupled to the microscopic cloud chamber, which is in turn coupled to the microscopic elementary particle. The identification of this particle is then interpreted in terms of the transfer of energy and momentum from the microscopic particle to a sufficiently large number of macroscopic particles within the cloud chamber that could register the occurrence of this particle within the human brain. In his analysis, however, the human observer (if he follows the present-day approach) ignores the influence that his macroscopic brain, eye, and cloud chamber may have on the motion of the (assumed free) elementary particle—i.e. the space-time trajectory of the particle is assumed to be uninfluenced by the presence in the universe of the human observer and his equipment. Mathematically, this corresponds, in the proposed theory, to a partial uncoupling of the field equations associated with the *observed* from the field equations of the *observer.* In this approximation, the formalism simplifies and takes the conventional form of ordinary quantum mechanics. On the other hand, when the *observer* and the *observed* have comparable inertial properties (such as a system of two elementary particles with comparable masses) no such approximation can be made. It is under such circumstances where the *exact* treatment of the system, as a whole, must be used and where one might expect significant differences in the predictions of this theory, compared with those of the conventional quantum field theory.

Since the measurement on the *observed* (which is the function of the *observer*) forms an integral part of the elementary interaction and *completes* its description, the theory is *deterministic.* From this view, quantum mechanics, as a formalism to describe the macroscopic measurement of microscopic systems, is interpreted as a statistical theory, in the sense of classical statistics. This is because the measuring process that is contemplated by the quantum theory is made up of a distribution of (completely determinable) elementary interactions rather than a distribution of (incompletely determinable) isolated elementary particle trajectories.

3 *The Maxwell-Lorentz Theory*

Two domains of macroscopic phenomena have been explained, quite successfully, by 'classical' field theories. One of these, the properties of gravitational fields, is predicted by Einstein's general theory of relativity. The other, the properties of electromagnetic fields, is predicted by Maxwell's theory of electromagnetism. However, there is one major difference in the domain of prediction of each of these theories. Einstein's general theory of relativity contains within its predictions the equations of motion of interacting masses. On the other hand, the Maxwell field theory does not predict the equations of motion of interacting charges. Thus, while the Einstein field theory does not necessarily require extension, additional physical assumptions and theory must be added to the Maxwell field theory if one is to insist that the 'particle' plays a role in the laws of Nature. It was Lorentz who joined the Newtonian concept of the mechanical particle to the field concept of Faraday and Maxwell. The resulting theory, called electrodynamics, has been successful in describing macroscopic electric and magnetic effects. However, as mentioned earlier, the consistency of the resulting Maxwell-Lorentz theory breaks down when we go from the macroscopic to the microscopic domain in the attempt to determine the intrinsic properties of charged elementary particles. Nevertheless, the Maxwell-Lorentz theory and its incorporation into a quantum field theory (quantum electrodynamics) has been used as a model from which to proceed in describing the other known types of physical phenomena that are important in the microscopic domain, e.g. nuclear interactions, weak interactions, and the other observed properties of a whole spectrum of 'elementary particles'.

We have no counterpart to the Maxwell field equations that would apply to nuclear and weak interactions. The reason, of course, for this imbalance in our understanding of Nature is that electromagnetic forces extend over all of space and are therefore easily probed. On the other hand, nuclear forces apparently become vanishingly small when the interacting bodies depart to separations greater than 10^{-13} cm. To probe distances of this order of magnitude requires very high energy experiments, not possible until recent years. In any case, as a consequence of extending the *field* approach of Maxwell to the *particle-field* approach of Lorentz and using its quantised version (which is necessitated by the underlying notions of the quantum theory) as a model for other types of phenomena, we do not have, at present, a consistent relativistic quantum field theory for any of the known types of physical phenomena.

The theory discussed in this paper attempts to resolve the difficulty by recognising the fundamental importance of the *elementary interaction* and by adopting the pure field description of Maxwell. Then, instead of adopting the Lorentz model, for extension to equations of motion, we describe natural phenomena, purely in terms of the coupling of *nonsingular* (i.e. finite and continuous) fields. (This also indicates a point of departure from the approach to the general relativistic consideration of interacting point masses as singularities in the field.)[1]

In view of the initial postulate regarding the fundamental nature of the interaction, rather than the free particle, there follows a new interpretation of the Maxwell field equations. The interaction is described in terms of the coupling of field variables that are associated with one of the particles with those of the other particle (or particles) that partake in the interaction. The description of electrodynamics is expressible in terms of two types of field variables. One set relates to the field intensities (associated classically with the electric and magnetic field variables) and the other set relates to the source fields (associated classically with the charge density and its motion). According to the new interpretation, Maxwell's equations are nothing more than a covariant prescription for determining the field intensity variables in terms of the source field variables or vice versa. Thus, *the Maxwell field equations are interpreted as a tautology*. The physical observable is not described until the field variables of one of the interacting particles in the system are coupled to the field variables of the other particles in a Lorentz invariant way. Such a coupling, in which either of the types of field variables that appear in Maxwell's equations may be used, describes a physical interaction between particles and it thereby transcends the information that is contained in Maxwell's equations.

With this interpretation, it follows that for each particle that is present in the system, there is a separate set of Maxwell's equations. It also follows that a particle cannot be allowed to interact with its own field—such a description would be redundant. The formalism, to be logically consistent with this feature, is then so constructed as to leave out those mathematical terms that represent a particle acting on itself. The disappearance of the self-energy terms automatically removes divergent quantities that appear normally in conventional quantum field theory and have their roots in the classical Lorentz theory. Thus, these divergences do not have to be *removed* as they are in the conventional theory (by the renormalisation procedure); they are not present from the outset!

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[1] Einstein discusses in *Ideas and Opinions* (loc. cit.) early attempts with N. Rosen to formulate a nonsingular description of matter that is based entirely on the field concept.

Two other important deviations from the conventional approach follow from the tautological interpretation of the Maxwell field equations. It follows that if there are no source fields then there can be no field intensities and vice-versa. Thus, this theory rejects the homogeneous solutions of Maxwell's equations as unphysical, i.e. only the particular solutions are acceptable within the framework of the theory. Hence, it follows logically that the concepts of the source-free radiation field and of the photon, as an elementary particle, must be rejected. This is because both of these follow from the homogeneous solutions of Maxwell's equations.

The photon concept is replaced here by the process of transfer of energy and momentum between systems of charged particles. As a consequence, it would follow, for example, that an atom in an excited state will not decay spontaneously, with the emission of a free photon. It will only emit a quantity of energy (a signal) if there is present in the universe another atom to absorb that energy. This general concept has already been discussed by several authors [22–24] and it is commonly referred to as *action-at-a-distance.*

While such a concept is logically sound within the framework of a covariant description, it appears (as remarked by Lewis [23]) to be 'repugnant to common sense'. Thus, with the standard 'particle' approach, one might make the following objection. Suppose that at a time t_1 an atom A, in an excited state, emits a quantity of energy to an atom B which is in its ground state and a distance R away. An observer would see the atom B absorb this energy at the later time $t_1 + R/c$. However, suppose that at the time $t(t_1 < t < t_1 + R/c)$ the observer removes the atom B from the universe. Since there is no longer an atom that is present to absorb the energy from atom A at the later time $t_1 + R/c$, the theory would say that atom A had no right to emit the energy in the first place. In other words, the implication of the *action-at-a-distance* approach is that when atom A emits a signal, it must have a knowledge of the future of atom B (i.e. the future of the entire universe!). The rejoinder would state that the *action-at-a-distance* concept is logically sound within a covariant formalism and that the example given is a subjective rather than an objective one (since it required the participation of the observer, disconnected from the universe of two atoms).

The aspects of the standard *action-at-a-distance* approach of the previous authors that may seem repugnant to common sense, are removed by this theory since it never relates to *isolated* atoms at all. According to this approach, the system of two atoms that was described above, would have a *closed* description with an associated *interaction field.* The latter prescribes nothing more than the interaction between the atoms and the constants of the motion that are associated with the *whole* system. This is, in fact, all that is necessary to compare with the measured properties of the system (i.e. the meter readings of the measuring apparatus). When we speak, as in the above example, of the process of transfer of energy between systems of charged particles, as a function of time, we refer only to the behaviour of the interaction field variable as a function of the parameter t. This parameter is, in turn, nothing more than a co-ordinate in a four-dimensional space which is used to map the field variables of the theory. Thus, there are no separate times t_1 and t_2 (in the exact description of this theory) for the separated atoms.

Finally, the similarity and the differences from the *action-at-a-distance* approach of Wheeler and Feynman [24] should be noted. The similarity is in the idea that any particle (or group of particles) will not change its (their) state of motion unless there is present in the universe other charged particles that would emit energy to or absorb energy from these particles. The differences are in the particle *vs* field approach as well as the 'free particle' *vs* 'interaction' approach. The Wheeler-Feynman theory describes the system of n particles as separate trajectories in a 4n-dimensional space. On the other hand, this theory assumes the existence of one interaction field (i.e. it is mapped on to a four-dimensional space). A difference in the consequences of the two approaches is that contrary to the Wheeler-Feynman approach, this one does not require an infinite absorber of radiation to surround the universe.

To elaborate further on the *interaction field*, we will now discuss the Dirac field variables of the theory—their new interpretation and the field equations from which they are derived.

4 *An Interpretation of the Dirac field*

With the premise that the field variables of the theory can be associated only with elementary interactions, the field equations for charged elementary particle systems have a fundamental form, not represented by the free particle Dirac equation, but rather by a pair of coupled Dirac-type field equations (for the simplest system—the two-particle system). In accordance with the imposed requirement of self-consistency (Sec. 2), the field equations have the covariant and self-consistent form [9]

$$o(1, 2)\, \psi^{(1)}(\mathbf{x}) = 0 \tag{1a}$$

$$o(2, 1)\, \psi^{(2)}(\mathbf{x}) = 0 \tag{1b}$$

where the operator $o(1, 2)$ depends on (2) through the field variable $\psi^{(2)}(\mathbf{x})$ and $o(2, 1)$ depends on $\psi^{(1)}(\mathbf{x})$ in a similar way, so that $\psi^{(1)} \leftrightarrow \psi^{(2)}$ implies that Eq. 1*a* ↔ Eq. 1*b*. Thus, the field equations are constructed to be invariant under the interchange of the field variable associated with the *observer* $\psi^{(1)}$ (or $\psi^{(2)}$) with that of the *observed* $\psi^{(2)}$ (or $\psi^{(1)}$).

Since $o(1, 2)$ depends on $\psi^{(2)}$ which in turn depends on $\psi^{(1)}$ (through Eq. 1*b*), it is seen that the fundamental field equations of the theory are *nonlinear*. Thus, the solutions $\psi^{(1)}$ and $\psi^{(2)}$ cannot be interpreted in terms of probability amplitudes as is done within the Hilbert space formalism of ordinary quantum mechanics.[1]

To satisfy the requirement of explaining the apparently discrete nature of microscopic systems, the nonlinear equations of the theory are so constructed that in the limits of (*a*) nonrelativistic energies, and (*b*) very large inertial properties in a part of the interacting system, compared with the rest of the system, the formalism reduces to the linear eigenvalue equations of ordinary quantum mechanics. Thus, the Schrödinger formalism and the Dirac formalism for atomic systems are regarded only as approximations to a nonlinear formalism. Along with the equations, however, it is also necessary to provide a new interpretation of the wave function, which does not give predictions that are different from the conventional theory (in the proper limits discussed above). We will now discuss this new interpretation.

The solutions of the field equations (1) (or the n-coupled field equations of the n-particle system) lead to a complete set of constants of the motion for the two-particle (or the *n*-particle) system. These are determined from expressions which follow from the invariance properties of the system of equations, i.e. from invariance under the set of *continuous* transformations that make up the proper inhomogeneous Lorentz group. There is, however, one other kind of observable that does not follow directly from the invariance properties of the formalism. This is a function that is directly related to the *weighting* of the interaction within the system of particles at all points of space-time. An important example of such an observation is the measured differential cross-section for particle scattering processes. [11, 29] In this case, one observes the magnitude of the unintegrated field amplitude for the system. Within the framework of the proposed theory, then, the cross section is not a measure of the probability of locating the physical particle in a particular region of space. It is rather a measure of the weighting of the interaction between the projectile particle, the target particle (or particles) and the measuring apparatus, *as a function of one set of co-ordinates.* The conventional probabilistic interpretation of the Dirac field variable is then replaced in the proposed theory by one interaction field that weights the elementary interaction in all of space-time. The field variables $\psi^{(1)}$ and $\psi^{(2)}$ are related to each other through the pair of field equations (1*a*) and (1*b*). An extension from the two-particle system to an n-particle system would then yield the field variables $(\psi^{(1)}, \ldots, \psi^{(n)})$ which are related to each other similarly through the n coupled nonlinear equations of the theory. In general, then, the existence of a single interaction field

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[1] See note 2, p. 224

$$\Psi(\psi^{(1)}, \ldots \psi^{(n)})$$

is postulated to provide a functional relationship between all of the solutions of the coupled field equations.

To interpret this interaction field as a weighting amplitude, it is required to satisfy the conservation law

$$\partial_\mu(\overline{\Psi}\gamma_\mu\Psi) = 0 \tag{2}$$

which is interpreted as the law of *conservation of interaction.* Such a conservation law is in accordance with the fundamental axiom of this theory. In the conventional way (i.e. using Gauss' theorem in four-space) Eq. 2 leads to a conserved quantity, which upon normalisation is

$$\int_{\substack{\text{all}\\\text{space}}} \Psi^\dagger\Psi\, d^3x = 1 \tag{2'}$$

Thus, the integrand $\Psi^\dagger\Psi$ is interpreted as a function that weights the interaction within the system of particles throughout one space-time.[1] It is indeed required in certain physical problems to determine the mapping of the interaction field itself in order to make comparison with experimental observations. Examples of the role that is played by the interaction field amplitude Ψ are given in the next three sections which relate to the observations that are interpreted as (i) the creation and annihilation of particle-antiparticle pairs, (ii) the Pauli exclusion principle, and (iii) the interpretation of the orbits of hydrogen.

5 *The Production and Annihilation of Particle-Antiparticle Pairs*

It follows from Eq. 2′ that the notion of the creation and annihilation of interacting particles must be abandoned. The former would increase the left-hand side of Eq. 2′ at arbitrary times while the latter would decrease it at arbitrary times. Thus, it is incumbent upon this theory to explain the experimental facts that are interpreted conventionally as the creation and annihilation of particle-antiparticle pairs, without actually creating or annihilating physical particles. This is done in ref. [9], where it is shown how an *exact* bound state solution of the field equations (1) for the particle-antiparticle pair leads to the experimental observations. This particular bound state exhibits dynamical properties that would be detected by other charged particles as two oppositely circularly polarised currents. (This is shown from the structure of the source fields in Maxwell's equations for this system as well as from the corresponding interaction field.) Such a state of the system is conventionally interpreted as the creation of a pair of oppositely circularly polarised photons that accompanies the annihilation process. Thus, in this theory, the photon does not enter into the description of the particle-antiparticle annihilation process. As we have discussed earlier (Sec. 3) this theory rejects altogether the concept of the photon as an elementary particle.

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[1] It should be noted that, unlike the conventional formalism, the field variables for an n-particle system are mapped on to a 4-dimensional, rather than a $4n$-dimensional space. The independent variables $\mathbf{x}$ in Eq. 1 are not tied to particle (1) and/or to particle (2). Rather, the point $\mathbf{x}$ denotes a space-time location where the *interaction* between the particles is described. Thus, this description replaces the conventional formalism that correlates individual particle positions with a description of one (interaction) field that is mapped on to one four-dimensional space. In the limits where the field equations can be linearised, one can consider the spaces of the 'noninteracting particles' separately, because of their uncoupling. Thus, for example, the field equations of this theory for an n-electron system, coupled to a nucleus, takes the conventional linearised form of the Schrödinger equation for the many-electron atom, in the nonrelativistic limit.

With the solutions $\psi^{(e-)}$ and $\psi^{(e+)}$ for this particular bound state of the particle-antiparticle pair, the invariance of the theory under *continuous* translations in space-time leads to a conserved energy-momentum that is zero (i.e. a null-vector). This state, therefore, corresponds to an energy (in the rest frame of reference) that is 2 mc^2 below the limiting state of two free particles, and is the ground state of the particle-antiparticle pair (i.e. the state that corresponds to the minimum energy-momentum). It is this particular state of the bound particle-antiparticle pair that replaces the *vacuum* state of the conventional quantum field theory. This is because it is out of this state that a pair is supposed to be ' created ' and into this state that a pair is supposed to be ' annihilated '.

It is clear that a transfer of 2 mc^2 energy units to this system from another (sufficiently energetic) charged particle system, by means of their electromagnetic coupling, can excite the pair out of its ground state and into the state that is conventionally interpreted as ' pair creation '. In this way, the experimentally observed ' pair annihilation and creation ' processes can be explained without actual recourse to changing the numbers of different kinds of particles in the universe at arbitrary times. Presumably, then, the universe should be populated with some large number of particle-antiparticle pairs in their ground states of null energy-momentum.

6 *The Pauli Exclusion Principle*

The next question that arises is: How, within the framework of a single field approach, does one interpret the Pauli exclusion principle, which appears to entail the correlation of positions and momenta of a many-particle system? To answer this question, consider the physical statement of the Pauli principle. Conventionally, this principle asserts that two equivalent particles (of a certain classification) cannot simultaneously be in the same state, i.e. they cannot be in the same location with the same constants of the motion. Thus, the conventional theory places a restriction on the separate probability amplitudes $\psi_m(\mathbf{x}_1)$ and $\psi_n(\mathbf{x}_2)$ of the respective trajectories of particles (1) and (2). (The indices m and n denote different states of motion of the respective particles.) In quantum field theory, the equivalent requirement on the operators for two different particle fields (at $\mathbf{x}_1$ and $\mathbf{x}_2$) is that their anticommutator vanishes identically when the mutual separation $|\mathbf{x}_1 - \mathbf{x}_2|$ is spacelike. This will ensure that two equivalent fermions cannot be simultaneously in the same state of motion.

Within the framework of the field theory discussed here, a result has been *derived* [10] that as a consequence of the self-consistency and full nonlinearity of the field equations (1) for like-particles, an interaction field $\Psi(\mathbf{x})$, satisfying the continuity equation (2), vanishes in all of space-time in the limit when the two particles (i) have the same inertial mass, (ii) have a mutually repulsive interaction, and (iii) are in the same state of motion.[1] Another way of stating this result is to say that two identical particles, each in the same state of motion, have an interaction that is unrelated to any observable.

This result has been generalised further to the case in which the two like-particles in the same state of motion are any two of a system that contains an arbitrary number of like-particles. In this case, it is shown that the interaction between two identical particles, each in the same state of motion, makes no contribution to the total weighting amplitude of the interaction field for the entire system (although their separate interactions with the rest of the system do contribute to the observables). The physical implications of this result, which required no additional axioms in its proof, are identical with those of the Pauli exclusion principle. It is also shown in ref. [10] how it follows from this property of the interaction field amplitude that its nonrelativistic limit takes the form of the antisymmetrised wave function of the Schrödinger theory for the many-particle system. The latter, of course, underlies Fermi-Dirac statistics, which in turn leads to the nonrelativistic results that deal, for example, with the buildup of the periodic table and the properties of metals.

7 *Interpretation of the Orbits of Hydrogen*

To illustrate further the interpretation of the interaction field, consider the nonrelativistic description of the hydrogen atom. The first successful model of the atom was proposed by N. Bohr. According to his model, the electron was to be considered as a classical point charge, moving in some definite orbit under the influence of the (central) Coulomb force of the oppositely charged point proton. With the further intuitive assumption about the quantisation of the action that is to be associated with the classical angular momentum of the orbital electron, Bohr was able to explain a large amount of data relating to the spectroscopic observations of atomic systems.

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[1] The state of motion of the i^{th} particle is described by the combinations of bilinear forms of the Dirac field variables $\overline{\psi}^{(i)}\gamma_\mu\psi^{(i)}$. Terms such as these are involved in the source fields of the Maxwell field equations. The i^{th} and j^{th} particles are in the same state of motion if $\overline{\psi}^{(i)}\gamma_\mu\psi^{(i)} = \overline{\psi}^{(j)}\gamma_\mu\psi^{(j)}$.

In the next stage of development, data on the atomic system that was unexplainable with the Bohr model, was given meaning with the application of quantum mechanics—a formalism that described the electron orbit, not by a discrete trajectory, but rather by a *field*, the solution of the Schrödinger wave equation for hydrogen. With the interpretation of this field variable (that was provided by M. Born) as a probability amplitude which locates the trajectory of the electron with a continuously varying probability as a function of the distance from the proton, improved agreement with the experimentally observed properties of atoms was obtained.

The nonlinear field equations of the new approach discussed here reduce, in the nonrelativistic limit, to the *mathematical form* of the Schrödinger equation for atoms. Nevertheless, the properties of hydrogen must be reinterpreted, within the framework of this theory, in terms of the interaction between the electron and the proton. This is in contrast to the consideration of the two particles as isolated *singularities*, perturbing each other at a distance. According to the interaction approach, the presence of the electron and the proton in the universe must be accounted for in terms of a *continuous field* that weights their mutual interaction and prescribes their influence on *each other.*

It follows from the solutions of the field equations that the electron-proton interaction is weighted most heavily in that region of space that is called the first Bohr orbit. Thus, no reference need be made to the electron and the proton as isolated entities. In this way, and in contrast to the conventional interpretation, the atom is described with a formalism that is based entirely on the field concept, and is in strict accord with the *principle of continuity*.

It should be emphasised again at this point that the discreteness of physical observables, such as the atomic energy level, is, within the framework of this theory, an *apparent discreteness.* It is only in an approximation to the exact nonlinear field equations of the theory that one arrives at the linearised eigenvalue equation for the atomic system—thereby leading to an apparent discreteness of atomic energy levels. It should also be remarked that another contributing factor to the limiting eigenfunction solutions of the field equations being the same as in the conventional theory are the boundary conditions on the weighting function amplitude, which are the same as those imposed on the wave function of ordinary quantum mechanics. Thus, the proposed theory predicts that atomic energy levels are not actually discrete and do indeed have a finite width associated with them, resulting from the nonlinear features of the field equations.

8 *The Electron Spin*

As the accuracy of experimental observations of the hydrogen spectrum improved further, it became apparent that the Schrödinger theory was not adequate. The reason was the lack of relativistic covariance of the theory. Dirac put the hydrogen theory in a covariant form (except that in the actual application of the theory, the approximation is used which assumes the proton to be infinitely massive compared to the electron, thereby giving the *central* Coulomb force to act on the electron). The result of this development was an improved agreement with the data.

The field variables of the Dirac theory revealed new degrees of freedom, associated with the particle, which were independent of its position and momentum. These were the variables that are associated with the intrinsic spin of the particle. In contrast, the interaction approach does not take the intrinsic spin as a property of an isolated particle—it is rather related to the transformation properties (associated with Lorentz covariance) that are applied to the field variables which are used to describe the interaction between particles.

9 *Negative Energy States*

Just as in classical relativistic mechanics, the Dirac equation gives rise to negative energy solutions. Thus, the Dirac theory predicts that the energy level spectrum of hydrogen lies in the range

$$-mc^2 \leq \mathrm{E} \leq mc^2$$

where m is the inertial mass of the electron. With the conventional interpretation, this means that there could be a transfer of energy of the order of $2mc^2$ between two hydrogen atoms or between one atom and any sufficiently energetic charged particle system. These processes would correspond to transitions between the positive and negative energy states of the atom. Such quantities of energy transferral (around one million electron-volts) between atomic systems are not observed in Nature, thereby indicating a serious defect in the Dirac theory. In the present-day quantum field theory, there are no negative energy states. This is a consequence of its formal structure and additional axioms regarding the nature of the vacuum state.

The lack of appearance of negative energy, as a physically meaningful entity, is also a feature of both classical relativistic mechanics and the theory discussed in this paper. The reason is the same. Recall that the energy-momentum of the 'classical' system is defined through the invariance of the physical laws under arbitrarily small *continuous* translations in space-time. To go from a positive energy solution to a negative energy solution would require a *discontinuous* transformation. Thus, while the solutions of the field equations reveal both positive and negative energy values, the proposed theory (or any 'classical' theory) can pick only the positive (or only the negative) energy solutions. By initially choosing a positive energy solution, all of the other energy values can correspond only to positive energy. The ground state of the system must, therefore, be greater than or equal to zero, i.e. a null vector. Thus, the arbitrary choice of positive energy solutions is made. Since the observations always refer to energy differences, it would have made no difference in the predictions of the theory, had the negative energy solutions been chosen. The important point to be made here is that there cannot be *both* positive and negative energy solutions, as a physically meaningful combination, in a classical field description of the atom (or any other elementary particle system).

10 *The Lamb Shift*

As the accuracy of measurements improved further, properties of the hydrogen spectrum were unveiled that were not even predicted by the Dirac theory. Thus, the observation of the Lamb shift—experimental evidence for extra energy levels in the spectrum of hydrogen—were not contained in the predictions of the Dirac theory. As mentioned earlier, present-day quantum electrodynamics gives predictions for these observations that are in remarkably close agreement with the data. However, in view of the criticism of the quantum theory of fields, and its scheme of prediction (which was discussed in Sec. 1) the investigation of an alternative *consistent* theory would be in order. The nonlinear field equations of the new approach discussed in this paper have been shown [9] to contain within its predictions, the hydrogen spectrum, *including the Lamb shift.* These results are arrived at in a self-consistent and unique way. At no stage of the calculations do infinite quantities appear, as they do in the conventional approach. We will now discuss, briefly, the origin of the term in the field equations that gives rise to the Lamb shift.

In accordance with the interpretation of the Maxwell field equations (Sec. 3), the idea of the photon, as an elementary particle, is abandoned by the present approach. As a consequence, it no longer becomes necessary to express the Maxwell field equations as vector field equations, if it is possible to express them in other covariant forms, especially if these other forms have a lower dimensional representation. It has been shown [12] that the Maxwell field equations (with sources!) can be factored into two uncoupled two-component spinor equations. It was this formulation of the Maxwell field that was used in the formalism of the new approach to electrodynamics. [9-11,33]

This factorisation of the Maxwell field, leading to two independent spinor equations, rather than the single vector equation, gives thereby a generalisation of the Maxwell formalism. The new formalism contains the same conservation laws and invariant quantities as the conventional Maxwell theory, *along with new conservation laws and additional invariants, that have no counterpart in the conventional formalism.* In the limit, when the ratio of the electron mass to the proton mass is taken to be vanishingly small, the field equations (1) approach the Dirac equation for hydrogen, except for the appearance of one extra term in the corresponding Hamiltonian operator. This term (which appears in a natural way in the theory) arises from one of the invariants of the generalised Maxwell equations that has no counterpart in the conventional formalism.

Because of a lower symmetry in this extra invariant, (it lacks reflection symmetry in the underlying space-time co-ordinate system) the accidental degeneracy in hydrogenic atoms is lifted—i.e. the Lamb shift is predicted.[1] (This is analogous to the prediction of the Zeeman effect in atomic spectra as a consequence of specifying the *axially symmetric* spin operator in addition to the *rotationally invariant* position operator, as the intrinsic variables of atomic electrons).

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[1] It should be noted at this point that, unlike the predictions of the conventional theory of quantum electrodynamics, the primary cause of the anomalous magnetic moment of the electron is not the same as that of the Lamb shift. It is due, in this theory, to the nonlinear aspects of the field equations that describe the coupling of the electron field to the field of a nucleus that participates in the observation. In earlier experiments [25], this nucleus was the binding proton in a hydrogen atom. In more recent experiments [26], the participating nuclei are in the polariser and analyser for a beam of polarised electrons that are trapped in a cyclotron. While the magnetic moment calculations have not been completed at this time, it can be stated that an

Thus, it has been shown that a complete and covariant field description of the electron-proton system does indeed give rise to the entire hydrogen spectrum, at least in a qualitative sense. The quantitative aspect of the Lamb shift calculation will be discussed in the following section.

11 *The Fundamental Length*

The additional term in the hydrogen equation depends on an extra fundamental constant that appears in the theory—*a fundamental length*. This constant appears in the Lagrangian formalism as a consequence of an unfolding from the vector form to the spinor form of the Maxwell field equations. The constant appears as a necessary multiplicative factor in the portion of the Lagrangian density which gives, under variation, the Maxwell field equations in their spinor form. Since the latter has the dimension of energy density per length, the new fundamental constant has the dimension of length. The fact that it is a 'length' has no other significance than this. (Similarly, the fundamental constant $\hbar$ appears as a multiplicative factor in that portion of the Lagrangian density that gives the Dirac field behaviour, in the limit of small interaction. Just as in the previous case, $\hbar$ has no other *fundamental significance* in this theory).

The Lamb shift has been measured in the excited states of hydrogen that correspond to the principle quantum numbers two [27] and three [28]. The *ratio* of Lamb shifts in these two states is significant here because its prediction, as a consequence of the formalism, is independent of the magnitude of the fundamental length (up to the accuracy that is required). Thus, an important successful result of the theory was the fact that the calculated ratio agreed with the observed experimental ratio, to within the accuracy of the data [9]. It was also interesting that the calculated ratios which involve the (yet unmeasured) Lamb shifts in the higher states of hydrogen, agree very well with the standard quantum electrodynamical calculations. However, it should be emphasised that the present theory is entirely different from the usual one (in regard

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important physical result of this approach is the implication that the anomalous part of the magnetic moment of an interacting electron should change (slightly) if the types of participating nuclei would be changed.

to both formalism and interpretation) and that in contrast with the latter, infinite quantities do not appear at any stage of the calculations.

After it was found that the (fundamental length-independent) ratio of measured Lamb shifts agreed with the calculations, the theory was used to determine the magnitude of this constant from the most accurately measured value. [27] Thus, it was found that the fundamental length had the value $(2.087 \pm 0.001) \times 10^{-14}$ cm.

Finally, the same formalism that describes the bound states of hydrogen was used to study the results of high energy electron-proton scattering. [11, 29] It was found that the cross section for the scattering event was indeed sensitive to the magnitude of the fundamental length. Using its value (that was determined previously from the Lamb shift) it was found that the prediction for the cross section behaviour with energy-momentum transfer came much closer to the data than does the prediction of the standard Mott theory for the relativistic coulomb scattering of charged particles with spin.

It should be emphasised here that no *a priori* arguments led to the explicit insertion of a fundamental length into the formalism. It emerged as a natural consequence of the formalism that describes electromagnetism. Nevertheless, the results of the electron-proton scattering calculation do imply an important role that is played by the fundamental length in the description of elementary interactions. The mathematical features of this study imply that with a continued increase in the transfer of momentum and energy between the interacting particles, a point is reached where the weighting of their mutual interaction (and consequently, the scattering cross section) cease to diverge and indeed start to decrease towards zero. This occurs when the corresponding mutual separation between the interacting particles approaches the order of magnitude of the fundamental length, i.e., the domain of sub-nuclear dimensions. Thus, within a domain of this magnitude, the elementary electrodynamical interaction is *effectively* reduced. This reduction continues (in a continuous fashion) towards zero as the mutual separation between the particles tends to zero (i.e. as the energy-momentum transfer becomes indefinitely large).[1]

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[1] It should be noted that other authors have also suggested that a fundamental length might appear in a general theory of elementary processes. [30, 31] One recent approach that explicitly introduces such a constant is due to Heisenberg. [32] The length appears in the latter theory, however, in an entirely different way from its mode of appearance in the present one. Heisenberg's approach is that of a *quantum field theory* (i.e. it maintains an underlying principle of linear superposition) which

One is led to conjecture, because of this result, that weak interactions (e.g. the forces that are responsible for the beta decay of radioactive nuclei) may actually be a manifestation of the electromagnetic interaction, in the region of sub-nuclear dimensions. This, of course, would be a desirable result within the framework of this theory, since its ultimate aim is to construct a universal elementary interaction field theory.

12 *Concluding Remarks*

In summary, the new approach to the theory of fundamental processes, that is based entirely on the field concept, permits a union of the results of the quantum theory with those of the theory of relativity —favouring the complete (deterministic) approach of the latter over the incomplete (nondeterministic) approach of the former. The essential point of departure from both the approach of the Copenhagen School and Einstein's approach to natural processes, lies in the introduction of the *elementary interaction* rather than the *elementary particle* as the fundamental entity of Nature. It may also be remarked that in this way, a covariant field theory of elementary particles is developed which is totally compatible with the *Mach principle.*

While the applications of elementary particle theory that were discussed in this paper have had to do only with the electromagnetic interaction, the general approach would, of course, apply to all types of forces that are encountered in Nature. An important extension of the formalism will be to gravitational phenomena. Thus, it is anticipated that the formalism could be enlarged so as to contain Einstein's gravitational field equations in a unified field theory. It should be emphasised, however, that the inertial mass of the unified theory could not play the role of an inherent property that is assigned to the isolated particle. Rather, it should be involved in the strength of the elementary interaction, together with the constant e^2. It is hoped that the solutions of the field equations of such a unified field theory could lead the way towards an explanation of the experimental results of high energy

attempts to derive the mass spectrum of elementary particles as a consequence of their self-energies. The necessary scale factor which multiplies the self-energy term in his nonlinear *field operator equations* must, for dimensional reasons, be a (squared) length. The present approach, on the other hand, does not admit the notion of *self-energy* as physically meaningful and it rejects the linear superposition principle as a starting point for a fundamental description.

physics that are interpreted as the mass spectrum of the elementary particles.

Recent investigations, in this direction, have commenced with a recasting of the generalised spinor form of electromagnetic theory into a generally covariant formalism. [33] There emerges from this development a generalisation of the conservation laws of the Maxwell theory. The (topological) invariants of the resulting formalism then relate to fundamental spinor forms that appear in the source terms in Einstein's field equations. The latter equations are expressed in a 'factored' form with quaternion fields, rather than the usual tensor form. In this way, the three manifestations of elementary interactions, in terms of the gravitational (Einstein), electromagnetic (Maxwell) and matter (Dirac) field descriptions, can all be represented in terms of fundamental spinor and quaternion field variables. It then follows (at least, qualitatively!) from the time reversal transformation properties of the underlying quaternion fields, that an eigenfunction equation emerges, whose eigenvalues can be identified with a spectrum of masses of spinor particles. [34] The quantitative predictions of these equations are presently under study.

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REVIEWS

Scientific Change. Edited by A. C. Crombie.
Heinemann, London, 1963. Pp. x + 896. £5. 5s.

THESE are the proceedings, including the papers, the commentaries, as well as the discussions of a symposium on the history of science that was held at Oxford university on 9th–12th July, 1961. They provide a most exciting and intriguing cross-section through contemporary thought on the history, and to some extent also on the philosophy of science. Indeed, it is gratifying to see to what extent an increasing interest in the evolution of scientific ideas, in the relation between science and philosophy has led to an 'enlarged and deepened conception of the history of science' in which the 'antiquarian's . . . passionate quest for the smallest detail' (Guerlac, 808 f.) is balanced (and, in the opinion of some, even overbalanced—see Guerlac 809, 811) by a concern for wider connections. There are nine parts dealing with antiquity; Chinese science; science and technology in the Middle Ages; sociology of science; modern science (three parts); history of science as an academic discipline; problems in the historiography of science. The present reviewer thinks that the following details will be of interest to a wider audience.

Part I. In the first paper L. Edelstein points out that 'at the very beginning of Greek science the individual scientist works in almost complete isolation' (19). He does not collaborate with others (19), and he is not supported, and perhaps not even recognised by society (27). The first feature, the individualism and dogmatism of the early scientists disappears with Plato and Aristotle. From now on the search for truth becomes 'a co-operative undertaking' (20). The second feature, the disregard, on part of society, for the scientist, is ascribed to the 'scientific situation as it had taken shape by the time of Hellenism . . . Rival systems . . . were competing with one another, rival systems which were in fact rival sciences. For there was nothing one could call science in the modern sense of the term, a body of knowledge, valid everywhere, a system of principles, of rules of procedure and of theories, well defined and generally accepted' (29). Only after the elimination of this variety, only after the second century A.D. had seen the arrival of a *scientia aeterna*, only then did society take 'a greater interest in science than the classical and the Hellenistic Age had done' (30). Recognition by society, institutionalisation, standardisation of teaching, in short, the arrival of many features which we today regard as characteristic of science, therefore seem to be contingent on unity of doctrine, and on a drastic restriction of debate. This conclusion has been drawn by Professor Kuhn in his contribution to part four of the symposium. I shall soon have occasion to present his argument. I shall then also have an opportunity to take issue with the *evaluation* that is implicit in Edelstein's paper and that seems to be made explicit in Kuhn's, viz. that a system of thought free from ' dissension ' (29) concerning fundamentals is more ' mature ' (Kuhn, 359), and therefore preferable to a situation where everything is open to criticism. At the present moment let me only point out that even the *facts* do not seem to be quite as Edelstein presents them. It may well be that those Athenians who criticised natural philosophy for its strangeness, its dogmatism, its incoherence, and who ridiculed it as an enterprise of which no one could say ' what [it] was and what it was all about, let alone . . . decide which of the existent systems should be encouraged and rewarded ' (Edelstein, 29)—were correct as regards the situation in Athens *at their own time*. What was defended here by the more recent *followers* of the earlier Presocratics, the already petrified doctrines of air, fire, and water may indeed not have made much sense. However it would seem to be somewhat unjust to extend such criticism to their predecessors also (cf. de St Croix, 81). There is reason to believe that the doctrines of *these* philosophers were developed not as isolated stand-points to be rigidly adhered to, but as tentative attempts to overcome recognised weaknesses of other systems. In the case of atomism this feature is clear and well documented. It does not seem to be too daring to extrapolate, and to assume that while the multitude of doctrines characteristic for later periods was a dogmatic multitude of unrelated beliefs, the theories of the Presocratics took cognizance of each other and tried to progress by a critical discussion of available alternatives (cf. also Vernant, 105 bottom). This interpretation which is due to K. R. Popper and which has been discussed independently by W. I. Matson will be of importance later, for the examination of Kuhn's paper. Papers by van der Waerden on the ideas and methods of Babylonian and of Greek astronomy, and by Sambursky on later Greek science conclude Part I.

Part II. Needham's survey of Chinese science is an excellent and brief introduction into a somewhat recondite field.

In the final paragraphs of the paper the author attempts to give a comparitive *evaluation* of Western science and Chinese science. The tendency to ' exalt unduly the role of the Greeks and claiming that not only modern science, but science as such, was characteristic of Europe, and of Europe only ' (140) is severely criticised. Moreover it is asserted that the astronomical application of Euclidean geometry ' was not all pure gain '—it ' ushered the Western mediaeval world into the prison of the solid crystalline celestial spheres—a cosmology incomparably more naive and borné than the infinite empty spaces of the Chinese *hun-thien* school or the relativist Buddhist philosophers ' (143). But even if we do not want to claim any *superiority* for Chinese science, we cannot *exclude* it by a one-sided reliance upon a set of values that is characteristic for Western science. We must recognise ' several kinds of values ' (176) and demand for the procedures of Chinese science a ' status as essential component of fully developed world science ' (145). As was to be expected this last part with its implicit reference to a ' spectrum ' of scientific procedures (an idea which was discussed later in the symposium and to which we shall return in due time) gave rise to critical remarks. Hartner defended the crystalline spheres (which in any case owe nothing to Euclid but quite a lot to the ' organic materialism ' of Aristotle) by pointing out that they were a very useful step in the development of astronomy (158)—and he is certainly right. For this idea, being more precise and therefore more fallible than the apparently much vaguer Chinese conceptions admitted of definite refutation and led thereby to concrete, and well-defined knowledge. Toulmin inquired whether in China there was ' the active recognition that heterodox ideas may actually be a *good thing* ' (170) and was assured (172) that this was indeed the case: ' the Taoist . . . had no objections to heterodoxy, being ' always ready to hear or see some new thing '. Now this is of course not sufficient for a science, mere tolerance leads to nothing *unless* it is combined with a comparative criticism of the ideas tolerated and with the attempt to improve them and make them the elements of a coherent point of view. In the last resort such debates concerning the value of a certain procedure can only be resolved by appeal to strict methodology—of which later.

Part III. For me the most interesting paper here was Lynn White's ' What Accelerated Technological Progress in the Western Middle Ages? ' This paper reports the results of detailed investigations cutting across traditional boundaries. Church dogmatics are analysed in order to establish the ideas contributing to the respective developments in the Greek and the Latin Middle Ages. This analysis is not allowed to stop at the letter. The historian, says White, is required to ' ask not only what [the authors] have said but the extent to which they really meant what they said ' (288). The function of the abstract phrase is therefore examined as part of actual life and it is expected that in this manner one will arrive at an ' historical understanding ' of specific phenomena of medieval technology, and especially of the surprising (in view of the existence of good communications) ' contrast between the technological mood and movements in the medieval West and those in its sister cultures of the Greek and Muslim East ' (273).

Among the remaining papers of this part we have one by Beaujouan emphasising the centrifugal elements in medieval thought—the techniques of scholastic disputation, the disagreement between authorities which forced the masters to take sides, the incompatibility of Aristotle's physics and Ptolemy's astronomy, the tension between the Aristotelian doctrine and some fundamental dogmas of the church. Southern in his commentary emphasises the ' bookish bias ' of education, grants ' even . . . Grosseteste ' the title of a scientist only ' in the sense in which Sir David Ross is a scientist ' (304) while Crombie agrees that ' to the extent that theoretical methodology was dissociated from attempts to solve scientific problems, theoretical quantification from actual measurement . . . mediaeval academic science . . . must appear with a built-in formula for frustration ' (323).

Part V commences with a paper by Buchdahl on the ' Logic of Scientific Discovery ' of Descartes. Buchdahl examines to what extent the hypothetico-deductive procedure was used by Descartes and how it was related to his apriorism. Hanson in his commentary warns not to ' throw the whole problem of discovery too quickly into those large, dark receptacles, called " intuition ", " inspiration ", " guesswork " and " hunch " ' (461) and remarks ' that the hypothetico-deductive elaboration is not always appropriate to describing how scientists actually reason towards their solution ' (464). As regards the latter remark it must be pointed out that the defenders of the hypothetico-deductive model have never intended it to be a description of this kind. Their problem is, and has been, the problem of justification and *not* the problem of discovery. As regards the warning it is to be admitted that to describe a new theory as the result of a ' hunch ' and to leave it at that would indeed be a caricature. Theories are hunches *intended to conform to certain conditions,* and among these conditions we find, apart from isolated observation reports, also other theories, general metaphysical beliefs, formal considerations, aesthetic prejudices and so on. Sometimes these additional conditions (which one might call the ' boundary conditions ' of the process of discovery) are sufficient almost completely to determine the structure of the new theory. However such determination is always either by deduction, or else in the form of an enlightened guess. Omit the boundary conditions, that is omit additional premises for deduction, and the only way to obtain a general statement is by a ' hunch '. The theory of the ' hunch ' therefore does not amount, as Hanson seems to suggest, to the unarticulated assertion that ' dark guesses ' are the only things operative in the process of the construction of a new theory. What it *does* mean is that to the extent to which deduction from ' boundary conditions ' leaves the theory undetermined, it must be guessed at. Deduction or guess—there is no third possibility. Daumas discusses the precision of measurement in the physics and chemistry of the eighteenth century. The paper contains a criticism of the idea ' that precision was an essential factor in the progress in [18th century physics and chemistry]' (426). Cohen in his excellent comments puts the question ' whether . . . an increase in precision is always, and necessarily, beneficial to the advance of science ' (467). He answers this question negatively by quoting examples where increased precision would have led to confusion, not to progress. Thus an increased precision in Tycho's observations would have revealed to Kepler ' that there are no planetary elliptical orbits save in an ideal Platonic sense !—in that case no Newton, no *Principia*, no law of gravitation . . . , and perhaps no modern science ' (468). Similarly the premature discovery of the variation of e/m with speed might have prevented the conclusion that electrons are the same in all atoms (469). Error thus does for the scientist what he very often must do for himself, it presents him with a *selection* from the ocean of possible observations which is *manageable* and may be ordered in a relatively *simple* fashion. It thereby guarantees a close connection between theory and experimentation—a fact that seems to be overlooked by the many contemporary hunters after more and more precise data. Moreover ' in physics often even simple measurement [must be] based on advanced theory ' (470) and if it is, then it is capable of producing interesting results (*vide* my remarks concerning the ' boundary conditions ' of discovery, above). Conversely, even the most precise measurements will not lead far if theory is not yet ready for them. Eighteenth century electrical theory did not remain qualitative only because it was a 'physique de salon ' which ignored measurement; rather . . . theory itself was not yet ready for measurement. At the century's end Coulomb did make precise measurements of the law of electric force, but from these results there flowed forth no striking theory. But the qualitative theory developed by Franklin and others, though not quantitative, and not based on measurement, was nevertheless *exact* in the sense that it yielded unequivocal and verifiable predictions about the outcome of laboratory manipulations ' (470 f.). Wilkinson's paper is short, but very interesting. It discusses those concepts of quantum mechanics which, although necessary for the theory, are yet in principle inaccessible to measurement. Examples are the amplitude of the wave function, the idea of a virtual state, the idea of the bare electron. Bohm in his reply criticises the current theory of measurement and suggests the outlines of a theory of his own. The current theory assumes, on the one side, that everything that exists is linked by indivisible quanta to everything else. On the other side an observed microsystem and an observing macro-system are regarded as being separable by a ' cut '. The resulting contradiction is kept from view by an ingenious method of interpretation which allows only the successful and unproblematic aspects of quantum mechanics to come to the fore. A detailed investigation of the processes linking the observer to the system he observes must therefore make use of a new approach that *contradicts* the orthodox theory in at least some respects (480 f.). The particular point of view discussed by Bohm starts with the notion of the total process of the universe ' which includes all that exists, . . . the natural world, man, his laboratory apparatus, etc.' (481). Individual objects are aspects which develop in the course of the process and whose relations to one another are determined by the particular way in which they are generated. Observation is a special ' kind of movement in which some aspects of the process are . . . " projected " into certain large scale results . . . [It] . . . is this process of projection [which] must be described by the theory [of measurement] ' (482 f.). This model of observation implies an interdependence of observer and object so that ' part of what we observe is a reflexion, or a projection of ourselves ' (483) and it is beautifully illustrated by the example of the observation of a mirror image (483 ff.). Hanson, in his criticism, points out that despite the shortcoming of the existing (or perhaps the *non*-existing? see his concession on p. 492, line 3) theory it is the best theory we have and should not be buried ' in the cemetery of dead and useless theories ' (493). Bohm has never suggested such gravedigging. Quite the contrary, some of his ideas such as the idea of a total process in which all objects are linked together are taken directly from the orthodox point of view. However they are developed by him in a consistent manner, and not applied only where application leads to easy success and dropped everywhere else. There is therefore no need to try to protect the quantum theory from Bohm and Vigier.

Part VI contains a most interesting paper by Ronchi on the history of the idea *non potest fieri scienta per visum solum* from antiquity up to, and including Kepler. This scepticism is explained as being ' largely due to the lack of understanding of the mechanics of sight '. It also ' rested on a broad knowledge of various optical illusions ' (552). It was therefore well justified and could be overcome, theoretically speaking, only by a comprehensive theory of vision capable of explaining to what extent illusions could be neglected. Galileo whose persistence contributed a great deal to the new and positive attitude towards vision does not provide such a theory. Quite the contrary ' he was totally ignorant of the science of optics ' (550) and had to rely on ' pure faith ' (552; cf. also 625). Imagine what this means! It means that relative to the contemporary background of theory, of general belief, of observable fact (existence of optical illusions; strange phenomena created by lenses) Galileo's telescopic results were of the same kind as the pronouncements of a medium who has been successful on some occasions, unsuccessful on others, whose trustworthiness cannot be supported by general arguments would appear to us today. (This example becomes even more relevant if we consider that Galileo had to insist that *his own* telescopes be used and had to reject the observations of others which did not verify his results—549 !) Is it not obvious that he would not have fared at all well with contemporary philosophers of science? And is it not equally obvious that a methodology which would have forbidden his procedure and regarded it as ' magical ' (see Farrell, 570—in this respect the Wittgensteinian Farrell is but the mouthpiece of his empiricist colleagues) is somewhat less than satisfactory?[1] Towards the end of his paper Ronchi uses the dependence of what is seen upon the physical situation as well as upon the perceptual apparatus as an argument for idealism. This is an unjustified extension of a valuable idea. Our perceptions, and even our physical concepts depend on our organisation. But some of these concepts and some of these perceptions have objective correlates and are to that extent pictures of reality—at least until further research has shown their inadequacy.

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[1] I have tried to develop a methodology capable of justifying such apparently irrational procedures in various papers, for example in ' How to be a Good Empiricist ' *Delaware Publications in the Philosophy of Science*, vol. ii, New York, 1963, as well as in ' Realism and Instrumentalism ' published in *The Critical Approach, Essays in Honor of Karl Popper*, Glencoe, 1964.

Time has now come to deal with the more specifically philosophic issues which were raised at the conference and with those papers which use historical details only accidentally. We already had occasion to comment on some arguments dealing with the 'nature of science'. Defending the scientific character of the achievements of the Chinese Needham had demanded to allow it 'status as essential component of a fully developed world science' (145) and had suggested the need 'for recognising several kinds of values' (176). In the debate about medieval science Crombie had emphasised the importance of seeing it 'in its historical context' (316) which was interpreted by some as an invitation not to criticise it when from our present point of view criticism would have seemed to be necessary. The attitude behind all these remarks came out into the open when at last the professional philosophers had their say. Farrell in his paper concerning the *Methodenstreit* in psychology invites us to recognise a 'spectrum' (575) of procedures and Alexander 'agree[s] with this' (616). Crombie comments on the 'variety of scientific methods' (5) and omnipresent Hanson criticises the procedure of 'importing pre-formed philosophical "isms" into a reading of the literature of science' (460) adopting here a position similar to the one he had outlined in his *Patterns of Discovery*. Farrell points to the relation between this New Liberalism (Let a Thousand Methods Bloom!) and 'Wittgenstein's use of language games' (568). This relation certainly exists—but it did not need a Wittgenstein to introduce the snake of relativism into the paradise of orderly scientific method. Wittgenstein only articulates (if that is the right word) in a very esoteric fashion an attitude which existed long before his time in many different fields (cf. again Guerlac, 802 ff.) and of whose independent existence he and his pupils do not seem to be aware. The attitude is as follows: procedures which are accepted by many, which form an essential part of their activity, which have proved to be a historical force (whether for the better, or for the worse—this does not seem to matter) are thereby *justified*; any criticism that might be directed against them is rejected as irrelevant, idealistic, as not being in accordance with the facts, as a flight from reality, as the erection of a 'castle in the air', to mention only some of the (more polite) terms which are used in this connection. But certainly we have not only the task to *know* the historical reality, we have also the task to *change* it. Should Weyer, Descartes, and others have rested content with the fact that the very detailed procedures of the *Malleus Mallefficarum* were followed to the letter by zealous priests and parishioners all over Europe, that they led to tremendous results (tens of thousands of witches burned and hundreds of thousands of souls thereby saved) and that they were therefore, according to the above attitude entitled to their legitimate place on the 'spectrum' of methods and 'forms of life'? Or was it not rather the case that an 'idealistic' criticism was needed of these procedures, a criticism that could not point to any *existing* results, to any already *effective* procedure, to any *popular* support but that did promise a better and a more enlightened future? Galileo's persistence in the face of what was established, effective, ' real ', is another example. These examples teach us what is obvious anyway, that a doctrine that affects the mind of all can still *be* outrageously inadequate although it will of course not be easy to *show* its inadequacy. This turn of the argument now brings me at long last to Professor Kuhn's original and provocative paper about the merits of a scientific dogmatism.

As opposed to what has just been argued it is Kuhn's contention that without a temporary ' closing of the mind ' (393), without ' a quasi-dogmatic commitment ' (249) to a certain point of view, ' mature science ' (359) would be impossible. The occasional dogmatism exhibited by individual scientists is not ' extraneous to science ' (348); it must not be reduced ' to the status of anecdotes ' (348); it is a ' precondition for success ' (349). The commitment to a particular point of view which may be sanctioned in a pragmatic as well as in an ' authoritarian ' (393) fashion, the defence of the ' historically accidental, the temporarily local, and thus of the arbitrary ' (393) ' is actually constitutive of research ' (349). And so are the corollaries of such a commitment, the resistance to novelties (348; 364); the authoritarian character of much of the education in the sciences (350); the unhistorical attitude which views the past either as being completely filled with darkness, or as already containing part of the present achievement (350). The argument in favour of this position of Kuhn's is partly historical, partly ' functional ' (359). The *historical argument* refers to the existence of two stages in the development of scientific knowledge, the ' pre-paradigm stage ' (354) which is not characterised by a unified doctrine but allows for unlimited disagreement, for the criticism of every and any assumption; and the stage of ' mature science ' (358) which is dominated by a single point of view and which presupposes an at least partial ' closing of the mind ' (393). The point of view dominating mature science brings about a concentration of effort. The ' foundations ' are now ' taken for granted ' and research is directed to the solution of ' more concrete and recondite problems ' (357). Being given an outline of the phenomena to be expected, the scientist ' largely ceases to be an explorer . . . or at least to be an explorer of the unknown. Instead, he struggles to articulate and concretize the known, designing much special-purpose apparatus and many special-purpose adaptations of theory for that task '. In short: ' mature ' science frees the scientists from the discussion of fundamentals ' to engage themselves with tiny puzzles ' (363).

The *functional argument* is twofold. On occasions the pre-paradigm stage is denied scientific status because it has not the features which we are accustomed to associate with ' science ' today. It is ' something less than " science " ' (355). This purely semantic argument which *condemns* an activity because it is not *customary* to apply a certain word to it looms large in

Kuhn's analysis (also in his *Structure of Scientific Revolutions*). However it is by no means the whole story. Kuhn also tries to show that the pedestrian activity associated with 'mature' science has a far-reaching effect upon the development of our ideas as well as upon their substantiality. This activity, this concern with the solution of 'tiny puzzles' leads to a close fit between theory and reality and it also precipitates progress. It does so for various reasons. First of all the accepted 'paradigm' of research gives the scientist a guide: 'as a glance at any Baconian nature-history or a survey of the pre-paradigm development of any science will show, nature is vastly too complex to be explored even approximately at random' (363). This point is not new. It was also made at the present symposium (see Cohen's comments reported above). The attempt to create knowledge needs guidance, it cannot start from nothing. More specifically, it needs a theory, a point of view that allows the researcher to separate the relevant from the irrelevant and that tells him in what areas research will be most profitable. To this idea Kuhn adds a specific twist of his own. He defends not only the *use* of theoretical assumptions, but the *exclusive choice* of one particular set of ideas, the monomanic concern with only one point of view. He defends such a procedure—and this leads to what I think to be the most important part of his philosophy—because he believes that its adoption will in the end lead to the overthrow of the very same paradigm to which the scientists had restricted themselves in the first place. If even the most concentrated efforts to fit nature into the current categories fail, if the very definite expectations created by these categories are disappointed again and again, then we are *forced* to look for something new. And we are forced to do this not just by an abstract discussion of possibilities which does not touch reality but is rather guided by our own likes and dislikes; we are forced to do this by procedures which have established a close contact with nature, and therefore in the last resort by nature itself. This is the main reason why the rejection, by mature science, of the pre-paradigmatic battle of ideas is defended by Kuhn not only as a historical fact, but also as a reasonable move.

Is this defence effective? That is, does a theoretical monism, the periodical occupation with 'tiny puzzles' indeed bring about what Kuhn says it does, a direct confrontation of theory and nature and consequently progress through revolutions from one set of concretely filled ideas to another such set, to still another set?

There are various reasons why I think that this question must be answered in the negative. Let me point out, first, that taken as a *factual* account Kuhn's description of 'mature science' is somewhat simplified (this point was also made by Hall, 370 ff. and acknowledged as correct by Kuhn, 387 f.). There may exist periods of mature science when one point of view does indeed reign supreme—but they are not the only periods. Nor are they very frequent. Towards the end of the last century there existed at least three different and mutually incompatible points of view. They were: the *mechanical point of view* which found expression in astronomy, in the kinetic theory, in the various attempts to devise mechanical models for the explanation of electric phenomena as well as (surprisingly) in the biological sciences including medicine; the point of view connected with the slow development of an independent and *non-mechanical theory of heat* which finally turned out to be inconsistent with mechanism; the point of view implicit in Maxwell's *electrodynamics* which was developed, and freed from its mechanical concomitants by Hertz. We may also mention, as a fourth alternative, the idea of a *purely phenomenological physics* as it was advocated by Mach (and was realised, to some extent by Bohr and his followers). It is quite correct that the kinetic theory was for many a metaphysical plaything and that Maxwell's theory experienced at first strong opposition, especially on the European continent. But still the idea that here we had a period governed by an all embracing paradigm which absorbed the physicist's attention to the exclusion of everything else is seen to be a gross over-simplification.

I have said above that Kuhn himself would admit that ' mature ' science is not an entirely monolithic enterprise but rather resembles a ' somewhat crazy, rambling structure ' (Hall 373, repeated by Kuhn 387). Yet he still emphasises the ' quasi-independent ' character of the various parts of it, each of which being ' guided by its own paradigms and pursuing its own problems ' (388). This brings me to my second point which is that the ' crises ' (367) terminating the reign of a certain paradigm are very often dependent on the *active interaction* of these inconsistent parts of ' mature science '. It was the active interaction of the various points of view described in the last paragraph which led to the overthrow of ' classical physics ' and *not* the dedicated exploration of a single paradigm (which one?) or the independent exploration of various paradigms. The troubles leading to the special theory of relativity could not possibly have arisen without an active exploitation of the tension that existed between Maxwell's theory on the one side, and Newton's mechanics on the other (Einstein has described this situation in beautifully simple terms in his autobiography). Nor was it possible to use the phenomenon of Brownian motion for a *direct* refutation of the second law of the phenomenological theory. The kinetic theory had to be introduced from the very start.[1] I quite agree with Kuhn that theories should be taken seriously and that one should not eliminate a good idea at the sight of the smallest difficulty. Criticism must be applied, after all, *both* to the theory, *and* to the statements describing this difficulty. But this plea for persistence, and this criticism of the theoretical butterfly who spends his entire life sitting now on the one, now on the other theory without ever advancing the cause of any is very different indeed from the combined historico-functional condemnation of a theoretical pluralism which we find in Kuhn's paper, and whose success would most probably be the end of ' the developmental pattern of mature science . . . from paradigm to paradigm ' (358). But let us assume, finally, that this pattern *can* perpetuate itself even if in between revolutions we are resolved to be stubborn monists and let us also assume that this is what has happened in the last 200 years. Does this mean that we have to reject the pre-paradigm procedure as being less ' mature '? Not at all! We can still *make a choice*. And considering the more undesirable features of ' mature science ', the dogmatic way in which it is taught, the fetters it puts upon the imagination, the way in which it turns (if Kuhn is right) bright young kids into not quite so bright older scientists, it is not at all certain that the choice would be in favour of the paradigm stage.

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[1] For details, cf. the first of the papers in the footnote on p. 249.

PAUL K. FEYERABEND

The Perception of Causality. By A. Michotte (translated by T. R. & Elaine Miles).
Methuen, London, 1963. Pp. xxiii+425. 45s.

MORE ink has probably been spilt in philosophy over the concept of causation than any other single concept. For Hume, for example, the causal relationship was a habit of expectancy; for Kant a relation of necessary connection, and for Maine de Biran a feeling of subjective effort. In recent years under the influence of phenomenology some writers, in particular the Gestalt psychologists and Michotte, have argued that we have a direct perception of causality, much in the same way as we perceive shape and movement. Michotte gives the example of a knife cutting a slice of bread. We do not, he says, just see two independent movements, the advance of the knife and the cutting of the bread: we have a specific causal impression of the two movements as spatially and temporally co-ordinated, forming a continuous process in which one is productive of changes in the other.

Michotte's great merit, however, is to bring the discussion of causality from a purely theoretical level to an experimental one. He found that causal impressions could be produced by using coloured shapes, or even images projected on a screen, and that they could be classified into two basic types, ' launching ' and ' entraining '. One of the more important pieces of apparatus used by him consisted of a partly concealed rotating disc bearing coloured stripes on a white ground. These show through a slit in a screen as two small coloured squares, one red, object *B*, is in the centre of the slit, the other black, object *A*, is a little to the left of *B*. In the launching experiment *A* moves towards *B* and stops when it comes in contact with *B*, while *B* then starts and moves away at a somewhat slower speed and then stops. The observer clearly sees object *A* bump into object *B* and send it off or launch it. In the entraining experiment, as soon as object *A* touches object *B*, *B* in its turn starts to move off at the same speed; the two objects continue together side by side. There is the definite impression that object *A* carries object *B* along or entrains it.

These causal impressions remained unaffected even when the same experiment was tried out hundreds of times on the same subjects. Only those who observed it in a highly analytical way saw the two movements. Michotte varied systematically such things as the size and shape of the objects, the temporal interval between action and reaction, etc. When certain conditions of speed, position and time-interval were satisfied his subjects received an impression of causal interaction between the two objects. For example, the movements of objects *A* and *B* have to occur in quick succession, and a change in *A* has to be followed by a change in *B*. As against Hume, Michotte shows, that habit and expectancy are not the crucial factors in giving rise to a causal impression. If the stimulus conditions are right, the causal impression occurs straight away; otherwise it will not occur however often the experiment is repeated.

In a further set of experiments Michotte studied the conditions under which moving objects look as though they were alive. He produced schematically the crawling movements of caterpillars and the swimming movements of fish. In the course of his experimental work he brought to light laws closely related to the Gestalt laws. One such law is that of ' ampliation ', namely the transmission of the movement of the active object to the passive one, He also makes an interesting comparison between his own views on causality and those of Hume, Maine de Biran and Piaget. Michotte is careful to point out that he is only concerned with phenomenological causality and not with epistemological questions.

Miles contributes a commentary. This includes a summary and notes, as well as two short but extremely interesting essays in which he gives a critical assessment of Michotte's work. In the first essay, ' Phenomenology and Scientific Method ', he points out that Michotte might be criticised for accepting a two-world picture, since he seems to distinguish phenomenal movement and causality from their physical counterparts, between which he assumes a high degree of correspondence. Miles believes that a conceptual reformulation of Michotte's work is possible, retaining the essentials but discarding the concept of phenomenology. We could avoid the two-world picture by saying that the verbal reports of the subject and the physical stimuli, were in a fairly consistent relationship. We can also use the subject's verbal responses to suggest hypotheses about the structure of events *x*, namely, the unknowns perceived by him, and these might be regarded as intervening variables. They can thus be studied without our having ' the " ontological discomfort " of having to regard events *x* as real existents ' (pp. 409-410). This neo-behaviourist formulation of Michotte's conceptual scheme involves, Miles argues, precisely the same experimental procedures, and in this sense the choice between them is arbitrary.

However, it seems doubtful whether the choice for Michotte is completely arbitrary. It is true that in an appendix Michotte points out that since the phenomenal world of the subject is inaccessible to us the events observed by him are unknowns, and that in interpreting his verbal responses, we can either adopt a behaviouristic point of view or a phenomenological one. Nevertheless, Michotte does not seem to regard these events merely as intervening variables or hypothetical entities. For example, he believes, that there are good reasons for accepting the hypothesis, that the verbal reports of the subject are descriptions of what he actually sees (cf. p. 311). We may perhaps save ourselves 'ontological discomfort' if we treat the events perceived by the subject as intervening variables. But since the whole object of Michotte's experiments is to demonstrate that there is such a thing as phenomenological causality, do we not completely defeat his purpose if we regard these events as purely hypothetical?

Michotte's phenomenology, though said to be different from that of Husserl's, does seem to have something in common with it. It is interesting in this connection, that these experiments were carried out at Louvain which is the centre of Husserl studies. Like Husserl and his school, Michotte seems to assume that in some way phenomenal causality is ontologically prior to our understanding of physical causality, although there is a correspondence between them. As he puts it, 'It is of course from phenomenal data that the physicists' world is constructed' (p. 307). It is therefore very doubtful whether he could be said to be putting forward a two-world theory. In his Autobiography (cf. *A History of Psychology in Autobiography*, vol. iii), Michotte tells us that there seems to exist in the phenomenal world, a sort of prefiguration of such abstract notions as substance, reality, causality, in terms of which the unsophisticated person spontaneously adapts himself to the world. And as Oldfield points out in his foreword to this book, for Michotte much of our everyday awareness of objects as permanent and real is dependent upon elements to which no physical stimulus corresponds at the time. 'It is a world of substances and things which, phenomenally at any rate, retain their permanence, their reality, their living or inanimate character, and which act causally on one another' (p. x).

In his second essay, 'Michotte's experiments and the views of Hume', Miles points out that nowadays, unlike in Hume's day, we distinguish philosophical questions about causality from psychological ones. As an example of the former he gives, 'Can sentences containing the word "cause" be replaced without loss of meaning by sentences containing the words "if. . . then"' (p. 410). This question, he tells us, is a conceptual one and cannot be settled by empirical tests, although empirical evidence may be relevant to the conclusions reached. Further, although Hume is psychologically incorrect in denying that a causal impression—in Michotte's sense—occurs, a Humean might still argue that all we see are two separate movements. 'The reason for talking in this way is that "I can see an x" entails in ordinary speech that an x is really there, and since all that is really there is two movements it is impossible as a matter of logic that we should see anything else' (p. 413).

We could even say, Miles goes on, that phenomenologically a causal impression sometimes occurs, whilst refusing to say that we 'actually see' causality. He believes, however, that a case could be made out conceptually for treating the word 'causality' as a suitable accusative for the verb 'see'. Thus, he continues, provided the phenomenology of the situation is not in dispute 'it is hard to see what useful purpose is achieved by discussing whether or not the causality is "really there" in addition to the two movements' (p. 413).

On such a view a sharp distinction is drawn between philosophical questions and psychological ones; the former are assumed to be purely conceptual in character, the later empirical. However, if we examine, for example, the question whether a causal statement can be translated into an *if... then* one, we see that a reference to an empirical context does come in as soon as we specify what sort of causal situation we have in mind. Further, if we concern ourselves with perceptual causality as described by Michotte this translation cannot be performed. We deal with more than a relationship of material implication. The cause is linked with the effect in a changing perceptual structure. As Miles points out, the translation of a causal statement into an *if... then* statement is only justified if we are not interested in perceptual organisation. No one would wish to deny that we can discuss the problem of causation on a purely conceptual level. But such a discussion, if it is to have a semantic content, requires a reference to physical or psychological data. An extensive knowledge of physical phenomena is, for example, required before we can proceed to formulate the concept of causality as it is used in physics.

To the question whether any useful purpose is achieved by discussing whether causality is really there as a Gestalt quality in addition to the two movements, we might answer that a good number of philosophical arguments would fail to satisfy the test of usefulness. The question which should perhaps be asked is, granted that such a quality is actually perceived, are we justified in attributing an ontological significance to it? Though the phenomenology of the situation may not be in dispute, its ontology may be. Michotte regards these causal impressions as direct, uninterpreted facts of experience. A Humean, on the other hand, although agreeing that the subject receives an impression of causality, would nevertheless regard it as a learned response. Further, if we take as self-evident the statement that all there really is are the two movements, then starting from such a premise it would, as long as we keep to logic alone, be formally impossible to see anything else. But for such a statement to have empirical meaning, we need to go beyond the formal discussion of its implications, to the data themselves. On this level, however, such statements can only be asserted with probability and not certainty.

Further, the question whether the word *causality* can be a suitable accusative for the verb *to see* would seem rather a grammatical than a philosophical one. As philosophers we ought not only to be concerned with the grammatical structure of the sentence, but also with its meaning, and this reflects our experience of things both past and present. In the light of such experience, we can ask whether it makes sense to say that we see *A* cause changes in *B*. We could, of course, discuss the existing grammatical usage of the verb *to see*, but is it even linguistically illuminating to do this? English grammar and usage did not develop in a void, but in a cultural and historical context.

In any case, traditional philosophical accounts of causation did make certain factual assumptions. A good example of this is Hume's appeal to experience when he attacked the view that causation is a necessary relationship. However, such an appeal seems largely to be based on the *a-priori* view that in the physical world events are distinct, and related after the fashion of balls drawn from an urn. On such a combinatorial model there is no intrinsic reason why any one event should be followed by any other. To explain why despite this we nevertheless have a direct awareness of causal efficacy, Hume had to introduce a feeling of expectancy based on past associations.

Michotte's account may not help us at all to understand how the concept of causality is used in present day physics, or even as it is discussed in contemporary epistemology. His researches are, however, of some importance if they are taken as illuminating our primitive perceptual experiences, in which our more sophisticated notions of causality presumably have their root. If we, however, try to see what sort of relationship there is between perceptual causality and the causality discussed in physical theory, epistemological questions do arise. Among those philosophers who have dealt with this question may be mentioned Whitehead, whose views on perceptual causality closely resemble Michotte's. Something of philosophical interest does emerge from Michotte's work. If we refuse to accept a sharp distinction between philosophical and psychological questions, certain traditional philosophical questions of a non-formal kind can be subjected to some sort of empirical study. For example, he has shown that it is simply not true that the habit of expectancy is at the root of our perception of causality.

Despite the undoubted value of Michotte's work, I must admit that I had a slight feeling of uneasiness when I saw some of these experiments demonstrated at Louvain. It was all a little unreal and made me think of a magician's parlour trick. This may perhaps be due to the divergence of what I saw from what I knew to be happening physically. In any case, the perceived world with which we are intimately acquainted, whether interpretation comes in or not, has a richer, more meaningful character than that depicted by a coloured spot pushing another coloured spot across a screen. This attempt to study phenomenological causality does seem to oversimplify the causal situation as we normally perceive it. Perhaps this is inevitable in any form of laboratory experiment. In attempting to discover the minimum conditions under which the causal impression occurs, Michotte has had to simplify and analyse our perceptual situation and perhaps in doing this something has been left out. Oldfield, for example, sees an outstanding paradox here, ' namely that impressions of causality in natural events do not always tally as regards their constitutive conditions with artificial ones in the laboratory ' (p. ix).

One final criticism of Michotte's work may be made. Despite his rejection of the view that interpretation enters into our causal impressions, this may yet occur, but in a completely unconscious way. Although our causal categories may have, as it were, some deeply rooted instinctive basis, we cannot leave out of account the part played by past experience and learing in our recognition of causal conditions. Some sort of causal schema in terms of which we come to see things is probably built up from childhood onwards, and may snap into action giving us an impression of causality when we find ourselves confronted by one of Michotte's laboratory experiments. As adults it is doubtful whether our perceptions are entirely free of some sort of conceptual interpretation. Perhaps the truth after all lies closer to Kant than Michotte or Hume.

This work which first appeared in 1946 has been competently translated from the second French edition of 1954. For the English edition Michotte has added an additional appendix in which he deals with more theoretical issues. He also contributes a preface together with an author's note. There is an illuminating foreword by R. C. Oldfield in which he gives a sympathetic, although not uncritical, appraisal of Michotte's work. Mr and Mrs Miles have prepared the translator's glossary of technical terms and a general bibliography of works on perceived causality. They have performed a valuable service in bringing Michotte's classical work before English philosophers and psychologists. This book should be compulsory reading in any university course in philosophical psychology.

W. MAYS

Understanding Physics Today. By W. H. Watson.
Cambridge University Press, 1963. Pp. xiii + 219. 30s.

PROFESSOR WATSON has written a book on the philosophical foundations of physics which can be read profitably from the physicist's point of view. The book sets no initial barrier for the physicist by discussing unreal situations or by falling short of grasping the complexity of problems as they actually present themselves to the worker in the field—a failing all too common in works on the philosophy of physics. To communicate effectively, indeed, with the physicist while succeeding in understanding physics from an original analytic point of view, is to make a contribution to fundamental physics itself. Hermann Weyl sometimes wrote so; and this is what Watson has achieved.

Early chapters set the stage for the main body of the book by clearing the ground of various emasculated accounts of what it is to do physics. A place is found for obstinate questioners of arguments that are basic to a given field of enquiry and that seem perfectly unexceptionable to other workers in that field; for the serious use of the imagination in guiding the operations of the formaliser; and for 'the metaphysical bias of ordinary language' (p. 44) in scientific discovery (provided—in this last case—that a critical attitude is maintained towards metaphysical forms of expression that happened to prove convenient in dealing with past problems, when we are confronted by new problems). On this last topic of metaphysical language Watson is touching on the main subject matter of his earlier book—the ancestor of the present one—'On Understanding Physics', which was the result of subjecting the thinking of the physicist to a type of analysis derived from the philosophy of Wittgenstein. This method still strongly influences Watson's detailed conclusions, especially—as we shall see later—in his readiness to separate problems out from one another and to allow each its own method of mathematical representation. However we should probably consider that the abstract discussions of 'modes of representation' that appeared in the earlier book have ripened, in the later, into actual pieces of theorising which can be discussed as physics independently of their philosophical origin.

From Chapter IV onwards the main topic of the book is more or less always in mind: how far ought we to think of an elementary particle as a piece of material which travels continuously from one place to another? A great deal has, of course, been written on this question. But the value of Watson's treatment of it is that he carries his enquiry right into the detailed mathematics of the various forms of quantum theory. The justifications we give of the detailed mathematical assumptions of quantum theory, and the estimates we try to make of the status of the substantial particle concept, are heavily interdependent, and unless we accept this fact we shall have to take over the mathematics of quantum theory as a monolithic construction (which it is not) and so sacrifice any possibility of effective and constructive criticism. The best we can then produce is a commentary on the application of an already completely prescribed framework of thinking.

Watson's reasons for believing that there must be a concept of a particle appropriate for use in quantum theory and definitely different from the corresponding classical concept follow familiar lines. As he puts it (p. 57)—'the particle differs from the objects of everyday experience because under no circumstances can we know of its presence without disturbing the environment of the particle in a physically significant way'. This is contrasted with classical mechanics which '. . . takes for granted the infinite possibility of refining our observation of the motion of objects in space and assumes the existence of operations which are logically necessary for the observing process, but which are nevertheless physically negligible'. He nails down the peculiarity of the experimental situation presented by the quantum particle (photon or massive particle) in much the manner of the classical account of Dirac.[1] An idealised experiment upon a photon with an interferometer is imagined which shows that if one thinks in terms of *states* of a system one has to consider the photon as being initially in a mixed state (partly in each of two states) and as being thrown suddenly into one or other of these states by the process of observation. There is no escape from the break with classical conceptions forced on us by this Gedanken experiment (and in particular a purely statistical interpretation of the quantum state is no escape, as Watson points out): it is accordingly justifiable in standard quantum mechanics to represent the mixed state by a superposition principle, and hence to introduce a second operation (addition) into the mathematics of quantum states.

The position established by Watson in this way sets the stage for his discussion. From it it follows that quantum theory is concerned with experimental situations in which every experiment must be started afresh with an informational blank sheet on to which we can fill in just those details of the entities appearing in the experiment that we actually infer from *that* experiment, and nothing more. In particular any object (such as the photon in the interferometer experiment) which serves to give us information about the experimental event must itself be part of the event: and if we want more information we can only obtain it at the expense of making it an essentially different event that we are investigating. Quantum theory, therefore, is committed completely to the view that it makes no sense to imagine a particle which moves continuously between the events that *have already appeared* in some description of *that* particle.

Now fundamental trouble appears. The position just described is in flat contradiction with the practice of physicists in a wide variety of experimental contexts, for particles behave in a much more classical way than they ought. Physicists continue to speak and act (as well as organise their experimentation) as though the classical notion of the substantial, continuously existing and continuously moving particle is correct, and there is therefore a head-on clash between practice and the basis of theory. Watson makes it quite clear that the apparent contradiction cannot be reconciled by just attributing the current practice to inertia on the part of physicists or to a temporary inability to find a better terminology. A cloud-chamber photograph (to take only the most familiar type of experimental evidence) is in effect a particle process in which certain major collisions are studied with the help of positional information given by a very large number of minor ones—the latter being strung out along lines which are spoken of as the tracks of particles. Now it is just not serious to deny that we are forced to postulate an entity which connects all these points, and if we have good reason to think (as we have) that it is pretty arbitrary which points on the track happen to be marked with water drops, then it is hard to see in what respects we have fallen short of describing all we need for classical motion. In such a situation the rigorous application of the theoretical principle illustrated by the interferometer Gedanken experiment is actually misleading. If, in the arrangement of cloud chambers shown below

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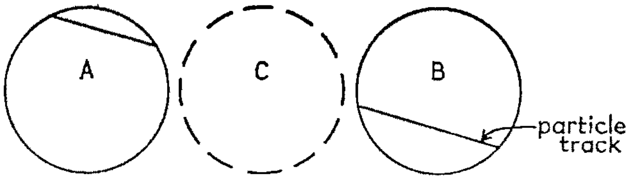
[1] *Quantum Mechanics*, Oxford, 1958, § 3



a ' particle ' is observed to travel across chambers A and B, we all know that *if* a third (dotted) chamber, C, *had* been present we should have seen the track across that too, and it is not reasonable in this case to refuse to consider the hypothetical extension of the apparatus to include chamber C on the ground that the experiments with chambers A and B and with chambers A, B, and C must be done separately and are therefore not to be considered one as part of the other. When referring to cloud chamber experiments Watson remarks—' These practices surely show that the claims by theorists about only treating what can be observed or measured are not consistent with the plausible reasoning of experimental scientists ' (p. 102).

To the reviewer, Watson's solution to the problem raised by the contradiction between practice and the basis of theory over the question of the continuity of particle motion is the main defect in his book: it is here that we see the effects of the tendency—mentioned earlier in this review—that seems to dog those deeply influenced by linguistic philosophy to pull the vital punch at the critical minute through an over-scrupulosity in preserving the diversity of problems. Study every case in its own right, Watson says in effect, and you will avoid perplexity. Speaking of the particular case of a moving particle, for example, he says: 'in the physical situation we must look at the circumstances of the case to see if we have reasonable grounds for speaking of the particle omitted from A having been detected at B.' His general discussion, given in a final chapter (' A view of atomicity '), follows the same lines moreover. For each experimental problem—Watson argues —the appropriate mathematical techniques have to be found, and having found them we have to realise that these techniques will have their own limitations and to avoid muddling ourselves by trying to push them further than they will go. ' It is when scientific discoveries seem to challenge formerly accepted ways of thinking about nature and ourselves that we are lured to philosophize as if by logic or rhetoric we could adapt ourselves to the new, either to accept it or reject it' (p. 213).

But now if the structure of mathematical quantum theory has to be understood as expressing a view of the nature of the particle that is *inconsistent* with the classical view, and if particles are *ever* found to require the classical view, then there is a flaw in quantum theory. Watson is surprisingly realist about the existence of atomicity in the world, and he thinks that there may be various mathematical modes of representation of this central phenomenon, but he cannot have one mode of representation that depends upon assumptions that are contradictory to the assumptions about atomicity of another mode of representation, and still keep both modes.

However Watson never draws the conclusion that quantum theory is inadequate in its representation of the particle: many times the text seems to lead the reader up to it, only to withdraw decently into some piece of sophistication, and a chance to make a clear cut impact on the course of physics—the fruit of careful preparation—is largely missed.

Watson's direct analysis of his central problem:—to what extent and in what circumstances a continuous 'atomic existence' (his phrase) can be said to connect events—by no means occupies his whole book (though it determines its form) for the analysis of this central problem leads to valuable discussions of the logical basis of the mathematical operations in several branches of quantum physics. Thus the more abstract and mathematical aspects of continuity and discreteness are dealt with at considerable length. Watson holds that continuous mechanics and discrete mechanics require essentially incompatible mathematical description. As he observes (p. 111): 'When we state that a numerical magnitude can assume only integral values we are already placing it in a system that presents the possibility of non-integral values.' He accordingly considers that a lattice (in the crystallographer's, not the logician's, sense), in which measurement reduces to counting operations, is the mathematical structure that properly represents the experimental basis of quantum theory (as distinct from that of classical mechanics). Watson then considers the complex field—as it appears in quantum mechanics—as a device by means of which this lattice of discrete numbers can be successively rotated into a continuum of equivalent lattices. A mechanics based on this device has the desirable feature that it includes the lattice and the continuum within one mathematical system. However continuity and discreteness are still incompatible as systems of mechanics, and since physicists demand a unified treatment they must be content with a formal unification in which there is a built-in notation for preventing us from interpreting quantities to which no experimental reality can correspond. The complex field provides this notation. Probably some physicists are additionally influenced by the verbal connection between 'imaginary' and 'uninterpretable', though of course they are muddled, for other fields isomorphic with the complex field exist which could equally well be used at the expense of a slight increase in complexity. Watson describes his position as follows:

> By means of the mathematical method, we seem to have assimilated the discrete operations of counting with the operations of physical measurement that lead us to continuous representation of the magnitudes measured. To represent quantum phenomena we have brought together counting and continuous measurement, and combined them in a single system in which we appear to have mixed up the discrete and the continuous. The latter is the natural mode for mathematical analysis. In it, counting must appear as a jump or discontinuous transition.

It has always been more difficult to understand the origin of complex numbers in quantum theory than it has been to explain its other basic mathematical features. Watson points out that even Weyl only offered ' mathematical convenience ' as an explanation of the origin of the complex field, and clearly considers that by his analysis of continuity and discreteness he has got to the root of the matter. And I think he has.

In the final chapters of the book there are valuable commentaries on the methods of quantum field theory, and on the theory of distributions as applied to quantum theory.

TED BASTIN

Philosophical Reasoning. By John Passmore.
Gerald Duckworth, London, 1961. Pp. ix+150. 15s.

PROFESSOR PASSMORE'S new book takes a close look at the peculiar ways in which philosophers, as opposed to mathematicians and scientists, reason. He believes that philosophy is characterised by its use of deductive argument of a peculiar character. This peculiar character consists in the conclusions

> not that a statement is false, but that it is ambiguous, meaningless, not a genuine contribution to the discussion; not that an argument is fallacious, but that it is not a proof, not an explanation, not a justification. In this resides their distinctiveness (p. 16).

In successive chapters the author examines the following types: the infinite regress; the impossibility of two-worlds interacting; the doctrine which refutes itself; the use of verifiability, excluded opposites (or polar contrasts), and paradigm cases to show meaninglessness; and the attempt to solve problems by allocation to categories. In each chapter the author takes a couple of examples of the argument in question and discusses them in detail. He aims to show ' on what suppositions they rest, how, if at all, they can be countered, and what sort of conclusions can depend upon them ' (p. 17). He also looks at their limitations, at what they can and cannot do. For example he shows that infinite regress arguments are destructive of conclusions, they do not lead to them. Passmore indicates that he has not covered all types of philosophical reasoning, but he does think that those he has selected out suffice to characterise what is peculiarly philosophical.

Written in the author's usual lucid style the book is entertaining and instructive. In at least two of the chapters Passmore goes further than his declared aim. In chapter three he comes to the conclusion that the two-worlds argument is strong enough to knock out any ontological dualism, and in his final, tentative chapter on categories, he argues that conventionalism does not solve the problem of giving criteria for class and category-membership. I would like to comment on both points.

Passmore holds that any mind-body dualism which postulates an ' ontological gap ' between mental and physical creates an insoluble problem because, being in separate existences, they cannot interact with each other. This seems to say no more than that if you are not careful you may postulate a mental and a physical world *so* different that there seems ' no possible way in which one could influence the other ' (p. 55). But this view of the unbridgeability of the ontological gap is based on the metaphysical theory that only ontological likes can interact and this is a theory which has been criticised, e.g. by Einstein. Since the theory is not incontestable I do not think Passmore has succeeded in showing that dualism is untenable. Of course we may not know what sort of interaction is possible but we cannot reject the possibility. Those who wish to hold open the possibility that unlikes might interact (after all their likeness is a function of our theories of similarity) can refer to a paper in this *Journal* (1952, **2**, 295-301), by its editor, which keeps open the *possibility* of bridging the gap.

Passmore's objection to a conventionalist approach to categories is that it involves a regress. If we say

> I intend to describe as ' logically false ' all sentences like, and only sentences like, ' my kangaroo is not a kangaroo '—we shall have to determine, and this was really our original difficulty, what sentences are like 'My kangaroo is not a kangaroo'.

The conventionalist solution would be purely arbitrary and useless. He says it is a real question

> not to be settled by decree, whether, for example, modal and deontic logics can be incorporated with traditional logic into a single generalized logic (p. 146).

What sentences are like ' My kangaroo is not a kangaroo' seems to me to depend upon one's point of view, or theory. As Passmore notes (p. 145), ' My kangaroo is green ' is like 'My kangaroo is not a kangaroo' in containing ' My kangaroo ' and ' is '. Yet, he says, it would be ' ridiculous ' to describe ' My kangaroo is green ' as logically false. It would or would not be ' ridiculous ' to the conventionalist depending only on the conventional way in which ' logically false ' was utilised. The word ' ridiculous ' will not bear the weight Passmore places upon it. A conventionalist would need a somewhat stronger argument to shake his belief that all classification problems are conventional. This may not be how the intuitionist sees matters but the issue is whether conventionalism answers Passmore's problem. For my own part I was, as Passmore correctly predicts, very impatient with his last chapter and all the working-over of definitions of category, category-difference and category mistake. After all, the categories are of language and surely in a philosophical sense *that* is conventional.

I. C. Jarvie

Philosophy, Science and The Sociology of Knowledge. By I. L. Horowitz. Charles C. Thomas, Springfield Ill., 1961. Pp. xiv+169. 62s.

Professor Horowitz writes:

> What I am interested in . . . are the conditions under which, and the social positions through which, knowledge is bent to serve interests first and truth second; and . . . the social forces and factors which enable these same human interests to be able to pursue the search for truth without bias (p. v).

He proceeds to review the history of the sociology of knowledge, tracing it through Taoism and Confucianism, Plato and Lucretius, Bacon and Hume, Malebranche and Helvetius, Kant, Hegel, and Feuerbach, to Marx.

The sociology of knowledge (all italics mine):

(*a*) examines the social channels through which knowledge . . . *is* filtered (p. 4);

(*b*) is concerned with knowledge from a social vantage point, that is, the accuracy with which social strata and economic classes *represent* the world (p. 7);

(*c*) has as a minimum base the attempt to eliminate *indeterminism* from the social context of idea formation (p. 7);

(*d*) [is] . . . epistemology in its social aspects . . . The environmental side of epistemology has given way to the empirical study of the social and *cultural determinants of knowledge*. How these *determinants* and their deviants help form the content and contours of human ideas is the essential field of the sociology of knowledge (pp. 35-6);

(*e*) investigates the ideational factors *compelling* men to act (p. 36);

(*f*) can be selectively summarized . . . to mean . . . (a) the empirical study of the ways ideas are used to galvanize action; (b) the social basis of ideas, irrespective of their truth or falsity; (c) the ideational conditions for the survival of social motivation; (d) the use of ideas and values to promote new forms of social relations, and the use of counter-ideas and counter-values to frustrate new norms; (e) the respective functions of science, religion, myth and logic in the creation of ideological conceptions of the universe; (f) the treatment of knowledge as an independent variable with its unique dynamics of change (pp. 37-38);

(*g*) has as its central task the social sources of truth, error, and opinion . . . (p. 68).

The sociology of knowledge being so many things may explain the comprehensive title of the book; it may further explain the claims that are staked for the subject.

I am afraid I am not clear as to what the problem is which started Professor Horowitz off on his book. Some of the difficulties one experiences with the book are due to the extremely abstract plane on which the argument is conducted. Much use of jargon, and no examples whatsoever. It looked to me like highly compressed lecture notes. In general Horowitz seems to want to resuscitate the sociology of knowledge. It seems a trifle odd to do this by wandering over the fields of philosophy, sociology, and methodology in a way that is likely to stir up more side-tracking controversy than would a straight presentation of some arguments as to why we should resuscitate the sociology of knowledge. Professor Horowitz finds space for remarks about determinism, about the history of philosophy, and about the nature of epistemology, on all of which the reviewer must eschew comment.

Does the sociology of knowledge need resuscitating, then, and if it does, will it be worth doing? Before I read this book my answer to both these questions was negative, so I relished the prospect of hearing the opposite case argued. As far as I could see the sociology of knowledge was popular, not dead; and were it dead I would have thought it best to count ourselves lucky, and not disturb the corpse.

I deplored the popularity of the sociology of knowledge because the very severe criticisms which had been launched against it were being ignored. Particularly the question: how is the sociologist of knowledge specially privileged in not being biased by *his* total ideology? Professor Horowitz affects to deal with this criticism, but so far as I can see his answer consists mainly in disowning Mannheim and his theory that the sociologist of knowledge must stand above all ideologies; Horowitz does this by modifying the Mannheimian idea of total ideology until it is not open to that criticism. Ideology, for Horowitz, no longer means ' class bias ', but rather

> the investigation of the social uses of ideas for the purpose of convincing or coercing men into actions which have ultimate political and economic consequences (p. 81).

After remarking that this seems less like a definition of ideology than of the study of ideologies, I still feel that if the sociology of knowledge is dead it better be left to rest in peace, despite Horowitz's belief in 'the practical impossibility of social science without it' (p. 64). My reason for this continued reluctance to have anything to do with the sociology of knowledge is simply because it dabbles in issues which are best not skated over. For example, having got rid of Mannheim's ' total ideology ' we are presented with another idea: the social determination of knowledge. But this seems to be predicated upon a vague notion of what epistemologists mean by ' knowledge '. Biases, or total ideologies, could be socially or economically determined, I suppose; but how can *knowledge* be? Knowledge is a discovery about the structure of the universe, how can it have ' determinants' ? It can only have them, I would argue, if by ' knowledge ' we mean ' theories '.

But this is first of all the sociology of theories (or opinions), not of knowledge. And secondly, what I have called the sociology of theories overlaps with the question of how human beings come by their ideas. But this is an issue which is very difficult to discuss as it involves problems like whether a theory of how we get theories applies to itself. Moreover, if the truth of our theories is independent of their origins, the issue is of no epistemological interest. It may be of psychological or sociological interest because if we could isolate the ideal circumstances for having ideas we could rebuild our social institutions.[1]

On one view, that of Bacon and Locke, the source of (true) ideas is observation, and the question of what the best conditions are for that is a psychological problem. Professor Horowitz's contention that

> truth does not spring up from the brain of its discoverer pure and pristine, free of distortion (p. 8)

suggests to me he shares a Bacon-Locke view. He presents no arguments to back this assertion. Those who reject the Bacon-Locke view but put nothing in its place tackle the problem by trying to give those interested in pursuing truth the maximum freedom and encouragement to proceed in whatever way they see fit.

In short, this book sets out to rehabilitate the sociology of knowledge, but it signally fails to do this or even to give us reasons for wanting to do so. I cannot think that its all-embracing manner has advanced the discussion at all.

I. C. Jarvie

Mathematical Logic and the Foundations of Mathematics. By G. T. Kneebone. Van Nostrand, London, 1963. Pp xiv + 435. 65s.

According to the jacket this book ' is designed as an introduction addressed primarily to two kinds of reader. The first includes honours undergraduates in mathematics, who need this topic as part of their degree course, high school mathematics teachers and those under training, and postgraduates specialising in some other branch of mathematics. All these require a book which covers the whole field, is informative, seriously written and substantial but not overloaded with technicalities. The second group consists of postgraduate students who want an introduction to mathematical logic or the philosophy of mathematics that will give them a correct orientation, a view of the entire subject and its literature, and quickly enable them to work from primary sources.' At present this audience is not very well catered for. Most existing books on mathematical logic (a notable exception being Beth's *The Foundations of Mathematics*) which attempt to be fairly comprehensive are either suitable only for the trained mathematician who can skim through pages of detailed calculation and extract the ideas behind them for himself or else represent a rather individual view of what is important rather than a balanced view of the whole field. Kneebone seems to have succeeded very well in producing a book for this audience. He has covered almost all branches of the subject and has been particularly careful to include historical notes and other background material which supply the reader with motivation. It would be difficult to produce a better book without restricting the audience; the student who wishes to specialise in mathematical logic would certainly wish to go into more of the technical details but Kneebone recognises this and suggests that when studying Part I the student should work through Hilbert and Ackermann's *Principles of Mathematical Logic*, and for Part II recommends Hilbert and Bernays's *Grundlagen der Mathematik* or Kleene's *Introduction to Metamathematics.* The non-mathematician would not wish for such a supplement and would be able to understand most of the present book—Kneebone himself suggests that this would include parts I and III and the broad outlines of the arguments in part II.

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[1] This topic, the structure of the world of learning, is an under-investigated question. Witness the continuing debate about the structure of research teams and institutes, of universities, of conferences, about the editing of journals and so on. Doubtless some professional sociological interest here would be welcome, but I can hardly conceive of it as orresponding to the traditional idea of the sociology of knowledge (Cf. quotation (*b*)).

Part I—Mathematical Logic has a short introductory chapter on traditional logic followed by chapters on the propositional and predicate calculus and a brief indication of the further developments. Part II—Foundations of Mathematics largely follows the historical development of mathematics and logic. Chapter headings are: The critical movement in mathematics in the nineteenth century. The logistic identification of mathematics with logic. Formalised mathematics and metamathematics. Gödel's theorems on the inherent limitations of formal systems. Intuitionism. Recursive arithmetic. The axiomatic theory of sets. Part III—Philosophy of Mathematics has chapters on : The epistemological status of mathematics. The application of mathematics to the natural world. Logic and the activity of thinking. There is an Appendix on developments since 1939 in the study of foundations of mathematics.

As one might expect from an author who has been for many years a practising mathematician in other fields there are discussions of some of the more mathematical aspects of the foundations of mathematics which are not usually found in this sort of text. The student of philosophy who does not know much mathematics (and most undergraduate students of mathematics too!) will be enabled to get a much better idea of the role of foundational studies in mathematics from, e.g. Kneebone's explanation of Peano curves, non-Euclidean geometry and the foundations of projective geometry.

A reviewer can always find something which he thinks he would have done differently or left out or put in. I would like to have seen for example more reference to the developments of Gödel's theorem and the representability of sets of integers within formal systems, also to the now vast literature on recursively enumerable sets. However the author might well argue that the last topic does not properly belong to the foundations of mathematics. On the whole a valuable book which should be equally useful for private study or as the basis for a course of lectures.

J. C. SHEPHERDSON

The Language of Science and the Language of Literature, 1700–1740. By Donald Davie.
Sheed and Ward, London, 1963. Pp. 87. 10s. 6d.

THE aim of this little book is to provide an alternative to what is described as 'the orthodox' view of the effect on literature of the scientific revolution of the seventeenth century. It is widely held that the mechanical outlook associated with the Newtonian philosophy was inimical to poetry and imaginative literature in general, and that the literature of the early eighteenth century became in consequence formal and lacking in feeling. Mr Davie, on the other hand, taking the view that poetry is written not with ideas but with words, holds that it was enriched by the acquisition of a new language which it could use metaphorically and so make available for its own purposes. For example, words like 'volatile', 'acrimony', 'insipid', 'astringency'—now used as often, if not more often, in relation to a person's temperament as in the chemical sense—were originally purely laboratory words ('philosophic words' was Dr Johnson's term for them), and when first used metaphorically had a pointedness and a force which they have now lost: they were then living metaphors in contrast to the dead metaphors which they have now become. Hence the automatic character of Augustan poetry is more apparent than real; its first readers must have been aware of a vividness to which we are denied susceptibility.

The point is well made, and the book is interesting and instructive. It may be questioned, however, whether the effect that Mr Davie points out is properly called a relation between literature and science—or at least whether that is not a particularised description of a more general characteristic of poetic activity, namely, that of giving any word, whether derived from scientific sources or not, an analogical rather than a literal meaning. At a time when science had just made such a tremendous forward leap, this characteristic would naturally be manifested very prominently in relation to 'philosophic' words, but, long before that time, words like 'disaster' and 'exorbitant' had passed into common speech from astrological ideas (Mr Davie confines his attention to chemical terms, but astronomy and physics would have afforded him many additional examples) and had become 'dead' metaphors. He is indeed rather inclined to overstate his case. For example, following Arthos, he traces the poet's practice of forming adjectives from nouns by the addition of a 'y', to a source in the writings of late-seventeenth-century scientists, and he suggests a similar association between such familiar indirect descriptions as that of 'finny tribes' for fish, and Linnaeus's binomial nomenclature in the classification of organisms. But Shakespeare's plays, for example, abound in instances of the former device—'the vasty deep', 'downy windows', 'the rooky wood' come immediately to mind—and Linnaeus (who, incidentally, was born after the death of Dryden, to whom Mr Davie ascribes the introduction of the indirect descriptions) was hardly likely to have been influenced by considerations of verbal felicity.

These are small points, however. Of more general interest is the evidence of co-existence of old and new ideas, characteristic of times of transition, revealed in the following quotation from Pope's *The Dunciad*:

> None need a guide, by sure attraction led,
> And strong impulsive gravity of Head;
> None want a place, for all their centre found,
> Hung to the Goddess, and coher'd around.

Mr Davie cites this to illustrate the double meaning—spiritual and material—of 'gravity', but surely a more striking significance is the immediate conjunction of the Aristotelian view of gravity as a predisposition of the falling body to reach its 'centre' (cf. Romeo's exhortation to his body to land in Juliet's garden—'Turn back, dull earth, and find thy centre out') and the Newtonian view of it as an 'attraction' exerted by the earth on an inert mass.

The book can be recommended to all whose interest in the relation of science to the humanities extends beyond the illusion that fundamentally there are 'two cultures'.

HERBERT DINGLE

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Campbell, K., 'One Form of Scepticism About Induction', *Analysis*, 1963, **23**, 80-83

Cunningham, L. P. C., 'Contradictory Assertions Convey Infinite Information', *Analysis*, 1963, **23**, 72

Donnellan, K. S., 'Necessity and Criteria', *J. Philos.*, 1962, **22**, 647-658

Ezorsky, G., 'Truth in Context', *J. Philos.*, 1963, **60**, 113-135

Feyerabend, Paul K., 'A Note on the Problem of Induction', *J. Philos.*, 1964, **61**, 349-353

Geach, P. T., 'Aristotle on Conjunctive Propositions', *Ratio*, 1963, **5**, 33-45

Harman, Gilbert H., 'How Belief is Based on Inference', *J. Philos.*, 1964, **61**, 353-359

Hillman, D. J., 'The Probability of Induction', *Philos. Stud.*, 1963, **14**, 51-56

Jackson, H., 'Note on the Terminology of Natural Deduction', *Analysis*, 1962, **23**, 48

Kaminsky, J., 'Ontology and Language', *Philos. Phenomenol. Res.*, 1962, **23**, 176-191

Kim, Jaegwon, 'Inference, Explanation, and Prediction', *J. Philos.*, 1964, **61**, 353-359

Levison, A. B., 'Foundations of Proof', *G. Metafis.*, 1962, **17**, 397-401

Mackie, J. L., 'Self-Refutation—A Formal Analysis', *Phil. Quart.*, 1964, **14**, 193-203

Mejbaum, Waclaw, 'Laws and Their Formulations', *Studia Filozoficzne*, 1964, **2**, 149-166

Molsil, G. C., 'Sur le principe du déterminisme', *Acad. R.P. Roumaine*, **4**, 33-39

Nelson, J. O., 'Are Inductive Generalisations Quantifiable?', *Analysis*, 1962, **22**, 59-65

Plochmann, G. K., 'Concepts and Bases in Körner's Logic', *Philos. Phenomenol. Res.*, 1962, **23**, 111-118

Pozner, A., 'Le problème de la contradiction dans le micromonde et la méthodologie du', *Vest. Moskov. Univ.*, 1961, **16**, 63-75

Putnam, Hilary, 'It Ain't Necessarily So', *J. Phil.*, 1962, **22**, 658-671

Quine, W. V., 'Implicit Definition Sustained', *J. Philos.*, 1964, **61**, 71-74

Ruja, H., 'The Present Status of the Verifiability Criterion', *Philos. Phenomenol. Res.*, 1961, **22**, 216-222

Schick, F., 'Consistency and Rationality', *J. Philos.*, 1963, **60**, 183-202

Szaniawski, K., 'O podstawowych formach wnioskowania statustucznegp', *Spraw. Nauk. spolecz. Pol.*, 1960, **3**, 109-110

Tatarkiewicz, K., 'Modèles et systèmes déductifs', *Dialectica*, 1962, **16**, 275-298

Wells, R., 'Is Frege's Concept of Function Valid?', *J. Phil.*, 1963, **60**, 685-743

Mathematics

Bouligand, G., 'Aux Lissières de la Mathématique', *Rev. Synth.*, 1961, **82**, 91-115

Dummett, M., 'The Philosophical Significance of Gödel's Theorem', *Ratio*, 1963, 139-155

Fiedler, H., 'Mathematick und moderne Logik', *Arch. Rechts-Sozialphilos. Dtsch.*, 1961, **47**, 553-568

Finetti, B. De, 'La decisione nell'incertezza', *Scientia*, 1963, **98**, 61-68

Grünbaum, A., 'Geometrie, Zeitmessung und Empirismus', *Archiv. für Philosophie*, **12**, 179-303

Kouznetsov, I. V., 'L'hypothèse en mathématiques' (in Russian), *Vopr. Filo.*, 1962, **16**, 82-93

Levy, P., 'Le fondement du calcul des probabilités', *Rev. Metaphys. Morale*, 1963, **68**, 25-56

Quine, W. V., 'Ontological Reduction and the World of Numbers', *J. Philos.*, 1964, **61**, 209-216

Wallen, C. J., 'Method in Mathematics', *Mod. Schoolman*, 1963, **40**, 139-161

Physical Sciences

Arias, J. M. R., 'Las fronteras de la cosmologia', *Est. filos. Esp.*, 1962, **11**, 277-306

Aldrich, Virgil C., 'A Point about Spaces', *Philos. Phenomenol. Res.*, 1964, **24**, 397-401

Chapiro, I. S., 'Sur la quantification de l'espace et du temps dans la théorie des particules fondamentales' (in Russian), *Vopr. Filos.*, 1962, **16**, 84-92

Clavelin, M., 'Galilée et la cosmologie traditionelle. La Première Journée du', *Rev. D'Hist. Sci.*, 1962, **15**, 1-26

Costa De Beauregard, O., 'Deux problemès en épistémologie du temps de la physique,' *Rev. internat. Philos.*, 1962, **16**, 358-376

Holton, G., 'Science and the Changing Allegory of Motion', *Scientia*, **57**, 1963

Hanson, N. R., 'On Counting Aristotle's Spheres', *Scientia*, **57**, 1963

Janosi, L., 'Analyse Philosophique de la théorie de la relativité restreinte', *Vopr. Filoz.*, 1961, **15**, 89-100

Jans, H., 'Causaliteit en quantum mechanica', *Bijdragen*, 1963, **24**, 79-91

Juhos, B., 'L'introduzione di ordini fittizi nei domini relativistici non univoci', *Riv. Filos.*, 1962, **53**, 403-437

Juhos, B., 'The Characterization of States of Translatory-Motion', *Ratio*, 1964, **6**, 28-49

Leyden, von W., 'Time, Number, and Eternity in Plato and Aristotle', *Phil. Quart.*, 1964, **14**, 35-52

Metz, André, 'La Notation atomique et la théorie atomique en France à la fin du XIXe siècle', *Rev. D'Histoire Des Sciences*, 1963, **16**, 233-239

Novik, I. B., 'La néguentropie et la quantité d'information' (in Russian), *Vopr. Filos.*, 1962, **16**, 118-128

Novacu, V., 'La méthode dialectique et les sciences physiques', *Acad. R.P. roumaine.* om.—4th, 71-92

Rocca, M., 'De la mécanique de Schroedinger à la chimie de Pauling', *Sophia*, 1963, **31**, 29-41

Sakata, S., 'Des notions nouvelles sur les particules élémentaires' (in Russian), *Vopr. Filos.*, 1962, **16**, 129-140

Sakata, S., 'L'interpretation de la théorie de la mécanique quantique', *Vopr. Filos. S.S.S.R.*, 1962, **16**, 132-142

Svidersky, V. L., 'La contradiction du mouvement mécanique', *Filos. Nauki, S.S.S.R.*, 1962, **5**, 83-89

Van Melson, A. G. M., 'Voorspelling en causaliteit', *Nederl. T. Psychol.*, 1961, **17**, 105-120

Biological Sciences

Barraicelli, N. A., 'Numerical Testing of Evolution Theories', Part II, *Acta Biotheoretica*, 1963, **16**, 99-126

Van der Klaauw, C. J., 'Verklaren begrilpen, waarheid en zekerheid in de biologie', *Alg. Nederl. T. Wijsbeg. Psychol*, 1962, **54**, 101-117

Social Sciences

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Farrell, B. A., 'The Status of Psychoanalytic Theory', *Inquiry*, 1964, **7**, 104-123

Kolenda, K., 'Unconscious Motives and Human Action' *Inquiry*, 1964, **7**, 1-12

Louch, A. R., 'The Very Idea of Social Science', *Inquiry*, 1963, **6**, 273-286

Martin, Michael, 'The Scientific Status of Psychoanalytic Clinical Evidence', *Inquiry*, 1964, **7**, 13-79

Martin, Michael, 'Mr. Farrell and the Refutability of Psychoanalysis', *Inquiry*, 1964, **7**, 80-98

Mora, J. F., 'The Idea of Man', *Lindley Lecture*, University of Kansas, 1961, 1-23

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Rotenstreich, N., 'The Philosophy in Psychoanalysis', *Ratio*, 1963, **5**, 82-90

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Thompson, J. W., 'Meteorological Models in the Social Sciences', *General Systems*, 1962, **7**, 283-291

Topitsch, E., 'Das Verhältnis zwischen Sozial und Naturwissenschaften', *Dialectica*, 1962, **16**, 211-231

Wilkins, Burleigh Taylor, 'Nagel's Critique of Psychoanalysis', *Philos. Phenomenol. Res.*, 1964, **24**, 383-396

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ERRATA

In the article 'Rational Deduction in Physics: The Parallelogram of Forces' by L. R. Saunders (1964, **14**, 265–73) an error occurs on page 270, line 28:

for $a_k = O(2m+1)$ read $a_k = o(2m+1)$

A correction is needed to the article 'On Foundations of Statistics' by Ian Hacking (1964, **15**, 1–26). Page 11, line 22 should read:

'that $D \epsilon \Delta$ and $E \subset E'$'

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Science in Early Victorian Thought . . . . . . *Walter F. Cannon*
Historicism in Dilthey, Troeltsch, and Meinecke . . . . *Calvin G. Rand*
T. G. Masaryk's Revision of Marxism . . . . . . *Erazim Kohak*
Peirce's Debt to F. E. Abbot . . . . . . *Daniel D. O'Connor*

NOTES

Social Roots of Heracliteanism . . . . *Bertrand Helm*
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Choosing	T. F. DAVENEY
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Tractatus 5.542	DAVID FAVRHOLDT
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